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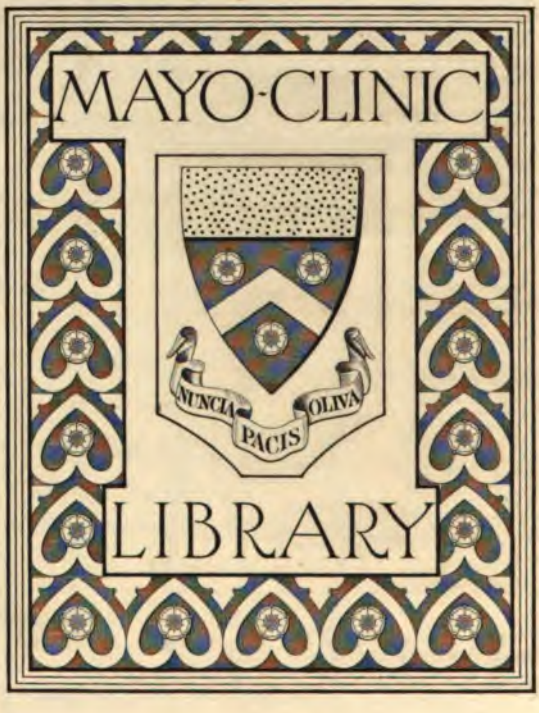

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NOTICES

OF THE

PROCEEDINGS

AT THE

MEETINGS OF THE MEMBERS

OF THE

Royal Institution of Great Britain,

WITH

ABSTRACTS OF THE DISCOURSES

DELIVERED AT

THE EVENING MEETINGS.

VOL. VI.
1870—1872.

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1872.

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No Copy of this Prospectus, printed in 1799, exists in the Library of the Royal Institution. Happily two copies have been preserved, the one at Althorp and the other at the British Museum.

Through the kindness of Earl Spencer the Managers have been able to order this very early Record of the Institution to be reprinted.

May 9, 1870.

PROPOSALS

FOR FORMING BY SUBSCRIPTION,

IN THE METROPOLIS OF THE BRITISH EMPIRE,

A PUBLIC INSTITUTION

FOR DIFFUSING THE KNOWLEDGE AND FACILITATING THE GENERAL INTRODUCTION OF USEFUL MECHANICAL INVENTIONS AND IMPROVEMENTS, AND FOR TEACHING, BY COURSES OF PHILOSOPHICAL LECTURES AND EXPERIMENTS, THE APPLICATION OF SCIENCE TO THE COMMON PURPOSES OF LIFE.

(Presented)

to

by the MANAGERS of the INSTITUTION.

INTRODUCTION.

THE slowness with which improvements of all kinds make their way into common use, and especially such improvements as are the most calculated to be of general utility, is very remarkable, and forms a striking contrast to the extreme avidity with which those unmeaning changes are adopted which folly and caprice are continually bringing forth and sending into the world under the auspices of fashion. This evil has often been lamented, but few attempts have been made to investigate its causes, or to remove them.

On the first view of the matter it appears very extraordinary indeed that any person should ever, in any instance, neglect to avail himself of an invention or contrivance within his power to obtain, that is evidently calculated to increase his comforts, or to facilitate his labour, or to increase the profits of it; but when we reflect on the

subject with attention, and consider the power of habit, and then recollect how difficult it is for a person even to perceive the imperfections of instruments with which he has been accustomed from his early youth, our surprise that improvements do not make a more rapid progress will be greatly lessened.

But there is a great variety of circumstances that are unfavourable to the introduction of improvements. The very proposal of anything new commonly carries with it something that is offensive;—something that seems to imply a superiority; and even that kind of superiority precisely to which mankind are least disposed to submit.

There are few, very few indeed, who do not feel ashamed and mortified at being obliged to learn anything new after they have for a long time been considered, and been accustomed to consider themselves, as proficients in the business in which they are engaged; and their awkwardness in their new apprenticeship, and especially when they are obliged to work with tools with which they are not acquainted, tends much to increase their dislike to their teacher and to his doctrines.

To these obstacles to the introduction of new improvements, we may add the innumerable mistakes, voluntary and involuntary, that are committed by workmen who are employed in any business that is new to them, and that perhaps they neither understand nor like; and (what is still more to be feared) those alterations which workmen in general, and more especially such of them as pride themselves on their ingenuity, have such an irresistible propensity to introduce when they are employed in executing anything that is new. How many useful inventions have been totally spoiled and brought into disrepute by what has been pompously announced to the public as improvements of them! And hence we may see of what infinite importance it would be to the progress of real improvements, to have some general collection of useful mechanical contrivances, constructed on the most approved principles, and kept constantly in actual use, to which application could be made as to a *standard*, in order to determine whether experiments which fail are owing to errors in principle, or to blunders of the workmen employed in the construction, or to those of the servants employed in the management of the machinery.

And how very useful would such a repository be for furnishing models;—and for giving instruction to artificers who may be employed in imitating them! Workmen must *see* the thing they are to imitate;—bare descriptions of it will not answer to give them such precise ideas of what is to be done as to prevent their being liable to mistakes in the execution of their work.

But this is also the case with mankind in general, and even with the best-informed; for how great must that effort of the imagination be, that is necessary to form any adequate idea of what we have not seen! Descriptions, though they be illustrated by the best drawings, can give but very imperfect ideas of things; and the impressions they leave behind them are faint and transitory, and seldom excite that

degree of ardour that ought to accompany the pursuit of interesting improvements.

Few indeed have an imagination so extremely vivid and susceptible as to become enamoured of a description, or of a picture. Something *visible* and *tangible* is necessary to fix the attention, and determine the choice.

But to return to the investigation of the causes that impede the progress of useful improvement. Besides those already mentioned there are several others which, though less obvious, tend nevertheless very powerfully to obstruct and retard that progress.

Those who propose improvements are commonly suspected of being influenced by *interested motives;* and this suspicion (which is often but too well founded) occasions little attention to be paid to such proposals by the public.

As the tacit recommendation of a respectable Public Institution, where the things judged to be worthy of the public notice would be *merely exposed to view*, would not be liable to this suspicion, it would certainly have more weight.

Not only suspicion, but *jealousy* and *envy* have often their share in obstructing the progress of improvement, and in preventing the adoption of plans calculated to promote the public good.

The most meritorious exertions in promoting the public prosperity are often viewed with suspicion; and the fair fame that is derived from those exertions, with jealousy and envy; and many who have too much good sense not to *perceive* the merit of an undertaking evidently useful, and too much regard for their reputation not to *appear to approve of it*, are often very far nevertheless from wishing it success.

This melancholy truth is, most unfortunately, known to everybody, and does more, I am persuaded, to deter sensible and well-disposed persons from coming forward into the public view with plans for useful improvements than all the trouble and difficulty that would attend the execution of them.

The managers of a public institution would be less exposed than an individual to the effects of these jealousies, and would no doubt have the courage to despise them.

In regard to those most important improvements that might in many cases be derived from the *scientific discoveries* of experimental philosophers, there are, unfortunately, many very powerful obstacles, which prevent their being as useful to mankind as they might be made, and as they would most certainly become, were those obstacles removed.

There are no two classes of men in society that are more distinct, or that are separated from each other by a more marked line, than philosophers and those who are engaged in arts and manufactures.

The distance of their stations—the difference of their education, and of their habits—the marked difference of the objects of their pursuits in life—all tend to keep them at a distance from each other, and to prevent all connection and intercourse between them.

The philosopher, who devotes his time to the investigation of the laws of Nature, must necessarily be independent in his circumstances, for he can expect no profit or pecuniary advantage from his labours; consequently he must be excited to engage in these pursuits either by curiosity or by a desire of fame, or by both these motives; and the nature of his occupations, as well as the intense meditation they require, naturally tend to detach his mind from all the common affairs and pursuits of life.

Anxious only to make new discoveries, and to establish his reputation among philosophers, whom he considers as the only competent judges of his merit, and whose suffrages alone can bestow that fame which he is ambitious to acquire, he has seldom either leisure or inclination to interest himself in those busy scenes in which the great mass of mankind are employed, and which he is perhaps but too apt to consider as being unworthy of his attention.

On the other hand, those who are engaged in arts and manufactures are seldom disposed to ask, or even to receive, the advice of men of science, with whom they have no connection, and of whose knowledge they seldom entertain any very high respect. Intent only on acquiring wealth, all their views are confined to that single object; and as their success depends much on their reputation for ingenuity in their different lines of business—as all proposals for introducing improvement presuppose some imperfection, such proposals are commonly not only considered by them as offensive, and rejected with disdain, but they frequently maintain that no farther improvement in their line of business is possible; except it be perhaps something they pretend to have found out, and of which, in order to enhance the reputation of their goods, they make a great mystery.

Ingenuity ought certainly to be rewarded. It is what every liberal-minded person would wish; but it is greatly to be lamented that the progress of real improvements should ever be obstructed by the effects of professional jealousies, or by any other of those selfish passions that are but too apt to influence men engaged in the busy scenes of life.

In making this observation, I would by no means be understood to call in question the wisdom of granting patents for securing certain privileges and advantages to the authors of new and useful inventions. So far from thinking this system of rewarding ingenuity disadvantageous to society, I am convinced that the present flourishing state of our manufactures, and consequently of our commerce, has been in a great measure owing to its operation.

I am only desirous that *science* and *art* should once be brought cordially to embrace each other, and to direct their united efforts to the improvement of agriculture, manufactures, and commerce, and to the increase of domestic comfort.

That the proposed Institution would facilitate and consolidate that union is too obvious to require any particular proof or illustration.

I shall mention only one circumstance more that may be assigned

as a cause for the slowness of the progress of new and useful improvements, and that is, the erroneous opinion that is but too generally entertained with regard to the real importance of what are called *improvements*, or their tendency to promote the happiness and prosperity of mankind. It is imagined by some that though a new invention may have some degree of utility, yet as our forefathers, who were not acquainted with it, contrived to do very well without it, so it cannot be a matter of any very great importance to us or to our posterity, whether it be brought forward into general use or not. But those who reason in this manner should be requested to recollect that all the successive improvements in the condition of man, from a state of ignorance and barbarism to that of the highest cultivation and refinement, are brought about by the use of *machinery* in procuring the necessaries, comforts, and elegancies of life, and that the pre-eminence of any people is, and ought ever to be, estimated by the state of *taste*, *industry*, and *mechanical improvement* among them.

Those among the inhabitants of this happy island, who have meditated profoundly on this interesting subject, will be very far indeed from being *indifferent* to the progress of improvement, and will certainly wish well to the success of the plan that is now laid before them; for they well know how powerfully the vivifying rays of Science, when properly directed, tend to excite the activity, and increase the energy, of an enlightened nation.

With regard to the *relative importance* of the different objects of improvements that are held up to view in these Proposals, nothing absolutely decisive can be determined. They are all very important, and there are, doubtless, many others perhaps equally so, that are not enumerated, that will, of course, in their turns, engage the attention of the Managers of the Institution.

It will not escape observation that I have placed the *management of fire* among the very first subjects of useful improvement, and it is possible that I may be accused of partiality in placing the object of my favourite pursuits in that conspicuous situation. But how could I have done otherwise? I have always considered it as being a subject very interesting to mankind; and it was on that account principally, that, at a very early period of my life, I engaged in its investigation; and the more I have examined it and meditated upon it, the more I have been impressed with its importance.

When we consider that arts and manufactures of every kind depend, directly or indirectly, on operations in which fire is employed, and that almost every comfort and convenience which man by his ingenuity procures for himself, is obtained by its assistance, we cannot doubt of its utility; and when we recollect that the fuel consumed in these kingdoms costs annually more than *ten millions* sterling, the great importance of every improvement that can be made in the management of fire must be quite evident.

To me, who am perfectly persuaded that *much more than half* the fuel that is consumed might very easily be saved, the subject must of

necessity appear very interesting, and on that ground I hope to be excused if I have dwelt upon it too long.

It may perhaps be not altogether uninteresting to those to whom I now more particularly address myself, to be made acquainted with the history of these Proposals, and of the causes which gave rise to them.

Having long been in a habit of considering all useful improvements as being purely *mechanical*, or as depending on the perfection of machinery, and address in the management of it, and of considering *profit* (which depends much on the perfection of machinery) as the only incitement to *industry*, I was naturally led to meditate on the means that might be employed with advantage to diffuse the knowledge, and facilitate the general introduction, of such improvements; and the plan which is now submitted to the public was the result of these investigations.

In the beginning of the year 1796 I gave a faint sketch of this plan in my second Essay; but being under a necessity of returning soon to Germany, I had not leisure to pursue it farther at that time, and I was obliged to content myself with having merely thrown out a loose idea, as it were by accident, which I thought might possibly attract attention.

After my return to Munich, I opened myself more fully on the subject in my correspondence with my friends in this country, and particularly in my letters to Thomas Bernard, Esq.,* who, as is well

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* Extracts of Letters written by Count Rumford to Thomas Bernard, Esq., from Germany:—

"MUNICH, 28*th April*, 1797.

"I feel myself very highly honoured by the distinguished mark of esteem and regard which the Society for Bettering the Condition of the Poor has conferred on me, and I beg leave through you to return the Society my respectful and grateful acknowledgments.

"This flattering proof of the approbation of those most respectable persons who compose the Society will tend very powerfully to encourage me to persevere in those endeavours to promote the important objects they have in view by which I first obtained their notice and esteem.

"I am very sanguine in my expectations of the good which will be done by this Society; they will, however, be able to do much more by examples—by *models* that can be seen and felt, than by anything that can be said or written."

"MUNICH, 13*th May*, 1798.

"The rapid progress you are making in your most interesting and laudable undertakings affords me a high degree of satisfaction. It proves that I was not mistaken when I concluded that, notwithstanding the alarming progress of luxury and corruption of taste and of morals in England, there is still good sense and energy to be found, even in the highest classes of society, where the influx of wealth has operated most powerfully. Go on, my dear sir, and be assured that when you shall have put *doing good* in fashion, you will have done all that human wisdom can do to retard and prolong the decline of a great and powerful nation that has arrived at, or passed, the zenith of human glory."

"MUNICH, 8*th June*, 1798.

"I have received your letter from Brighton of the 12th ult. You can hardly imagine the high degree of pleasure and satisfaction which I feel at your success

known, is one of the founders and most active members of the Society for Bettering the Condition and Increasing the Comforts of the Poor.

This gentleman I found, on my return to England in September last, not only agreeing with me in opinion in regard to the utility and importance of the plan I had proposed, but very solicitous that some attempts should be made to carry it into immediate execution in this capital.

After several consultations that were held at Mr. Bernard's apartments in the Foundling Hospital, and at the house of the Lord Bishop of Durham, at which several gentlemen assisted, who are well known as zealous promoters of useful improvement, it was agreed that Mr. Bernard should report to the Committee of the Society for Bettering the Condition of the Poor the general result of these consultations, and the unanimous desire of the gentlemen who assisted at them, that means might be devised for making an attempt to carry the scheme proposed into execution.

The gentlemen of the Committee agreed with me entirely in the opinion I had taken the liberty to express, that the Institution which it was proposed to form would be too conspicuous, and too interesting and important, to be made *an appendix* to any other existing establishment, and, consequently, that it must stand alone, and on its own proper basis; but as these gentlemen had no direct communication

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in your most laudable undertakings. Go on, my dear sir, and be assured that you will contribute more essentially to the revival of taste and morals, of energy, industry, benevolence, and *prosperity* in your favoured country than all the speculators and reformers in the three kingdoms.

"When society is arrived at a certain degree of torpid indifference and enervation of mind and body, which are the unavoidable effects of wealth, luxury, and inordinate indulgence, mankind must either be *allured* or *shamed* into action. Precepts and admonitions have no effect on them.

"As they are too indolent to take the trouble either to investigate or to choose, they must be led to acts of useful benevolence as they are led in everything else —by *fashion;* when you shall have rendered it perfectly ridiculous for a man of fashion and fortune *to have the appearance* of being insensible to the most noble and most delightful of human enjoyments—that which results from doing good—you will have done more for the relief of the poor than all that the Poor Laws can ever effect. Deeply impressed with the necessity of rendering it *fashionable* to care for the poor and indigent, and contribute to their relief and comfort, in order to diffuse in England that spirit of active benevolence you are kindling, I am apt to insist, perhaps with too much prolixity, on that important point.

"I am anxious to hear of the execution of your plan with regard to Bridewell. A well arranged House of Industry is much wanted in London. It is indeed absolutely necessary to the success of your undertaking, for there must be something *to see* and *to touch*, if I may use the expression, otherwise people in general will have but very faint, imperfect, and transitory ideas of those important and highly interesting objects with which you must make them acquainted in order to their becoming zealous converts to our new philosophy, and useful members of our community. Pray read once more the 'Proposals,' published in my second Essay. I really think that a public establishment like that there described, might easily be formed in London, and that it would produce infinite good. I will come to London to assist you in its execution whenever you will in good earnest undertake it."

with any persons, except with the members of their own Society, they appointed a Committee, consisting of eight persons, from their own body, to confer with me on the subject of my plan.*

I had the honour to meet this Committee on this business on the 31st of January, at the house of Richard Sulivan, Esq., where a plan I had previously drawn up, for forming the Institution in question, was read and examined, and its principles unanimously approved; but as some of the gentlemen present were of opinion that the plan entered too much into detail to be submitted to the public in the beginning of the business, I undertook to revise it, and to endeavour to accommodate it to the wishes of the Committee.

Having made such alterations in it as I thought might satisfy the Committee, I sent a corrected copy of it to them, accompanied by the following letter:—

"GENTLEMEN,

"Inclosed I have the honour to send you a corrected copy of the Proposals I took the liberty of laying before you on Thursday last, for forming in this capital, by private subscription, a Public Institution for diffusing the knowledge and facilitating the general and speedy introduction of new and useful mechanical inventions and improvements; and also for teaching, by regular courses of Philosophical Lectures and Experiments, the application of the new discoveries in science to the improvement of arts and manufactures, and in facilitating the means of procuring the comforts and conveniencies of life.

"The tendency of the proposed Institution to excite a spirit of inquiry and of improvement amongst all ranks of society, and to afford the most effectual assistance to those who are engaged in the various pursuits of useful industry, did not escape your observation; and it is, I am persuaded, from a conviction of the utility of the plan, or its tendency to increase the comforts and enjoyments of individuals, and at the same time to promote the public prosperity, that you have been induced to take it into your serious consideration. I shall be much flattered if it should meet with your approbation and with your support.

"Though I am perfectly ready to take any share in the business of carrying the scheme into execution, in case it should be adopted, that can be required, yet there is one preliminary request which I am desirous may be granted me, and that is, that the Government may be previously made acquainted with the scheme before any steps are taken towards carrying it into execution; and also that His Majesty's Ministers may be informed that it is in the contemplation of the founders of the Institution to accept of my services in the arrangement and management of it.

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* The gentlemen chosen were, The Earl of Winchelsea, Mr. Wilberforce, The Rev. Dr. Glasse, Mr. Sullivan, Mr. Richard Sulivan, Mr. Colquhoun, Mr. Parry, and Mr. Bernard.

"The peculiar situation in which I stand in this country, as a subject of His Majesty, and being at the same time, by His Majesty's special permission, granted under his royal sign manual, engaged in the service of a foreign prince, this circumstance renders it improper for me to engage myself in this important business, notwithstanding that it might perhaps be considered merely as a private concern, without the knowledge and the approbation of the Government.

"I am quite certain that my engaging in this, or in any other business in which there is any prospect of my being of any public use in this country, will meet with the most cordial approbation of His Most Serene Highness the Elector Palatine, in whose service I am; for I know his sentiments on that subject; and although I do not imagine that His Majesty, or His Majesty's Ministers, would disapprove of my giving my assistance in carrying this scheme into execution, yet I feel it to be necessary that their approbation should be asked, and obtained; and, if I might be allowed to express my sentiments on another matter, which, no doubt, has already occurred to every one of the gentlemen to whom I now address myself, I should say that, in my opinion, it would not only be proper, but even necessary, to inform Government of the nature of the scheme that is proposed, and of every circumstance relative to it, and at the same time to ask their countenance and support in carrying it into execution; for although it may be allowable, in this free country, for individuals to unite in forming and executing extensive plans for diffusing useful knowledge, and promoting the public good, yet, it appears to me, that no such establishment should ever be formed in any country without the knowledge and approbation of the Executive Government.

"Trusting that you will be so good as to excuse the liberty I take in making this observation, and that you will consider my doing it as being intended rather to justify myself, by explaining my principles, than from any idea of its being necessary, on any other account, I have the honour to be, with much respect,

"Gentlemen,

"Your most obedient and

"Most humble Servant,

(Signed) "RUMFORD.

"BROMPTON ROW,* 7*th February*, 1799."

(Addressed)

"To the Gentlemen named by the Committee of the Society for Bettering the Condition of the Poor to confer with Count Rumford on his scheme for forming a new Establishment in London for diffusing the Knowledge of useful Mechanical Improvements, &c."

The Committee above-mentioned having, in the mean time, made

* (Thursday.)

their Report to the Society for Bettering the Condition and Increasing the Comforts of the Poor, that Society came to the following Resolution:—

"At a meeting of the Society for Bettering the Condition and Increasing the Comforts of the Poor, on Friday, the 1st of February, 1799,

"PRESENT,

The Bishop of Durham, in the Chair,
Patrick Colquhoun, Esq.,
Thomas Bernard, Esq.,
William Manning, Esq.,
John Sullivan, Esq.,
The Rev. Dr. Glasse,
John J. Angerstein, Esq.,
William Wilberforce, Esq.,
Richard Joseph Sulivan, Esq.,
Matthew Martin, Esq., Secretary,

the Committee appointed to confer with Count Rumford reported, that they had had a conference with the Count, and that they were satisfied that the Institution proposed by him would be extremely beneficial and interesting to the community; that in order to provide the pecuniary funds of the Society at its commencement, it was proposed that subscribers of fifty guineas each should be the perpetual Proprietors of the Institution, and be entitled each to perpetual transferable tickets for the Lectures, and for admission to the apartments of the Institution; and that as soon as thirty such subscribers offered, it was proposed to call a meeting of those thirty subscribers, in order to lay the plan before them, and elect Managers for the Institution.

"Resolved,

"That the said Report be approved of, and that it be referred to the gentlemen of the Select Committee to communicate the outlines of the plan to the members of the Committee of the Society, and to such other persons as they shall think fit, desiring that those who wish to have their names inserted among the original subscribers to the Institution would communicate their wish to the Special Committee.

"Extracted from the Minutes,

"M. Martin, Secretary."

In consequence of this Resolution, a paper was printed by the gentlemen of the Select Committee, containing the outlines of the plan, and sent round privately among their friends, and others whom they thought likely to countenance the scheme, accompanied by a printed copy of the foregoing Resolution, with a request that those who were willing to allow their names to be put down among the original subscribers and Proprietors of the Institution would be so

good as to communicate their intentions by a letter addressed to Thomas Bernard, Esq., at the Foundling.

The Proposals that were circulated in this manner met with so much approbation that fifty-eight of the most respectable names * were sent in before measures could be taken for holding a meeting, and these successful beginnings encouraged those who were principally concerned in forming and bringing forward this plan to make some alterations in it, and particularly in respect to the time and manner of choosing the first set of managers, and in regard to an application for a charter for the Institution which it has been determined to make in order to place the establishment on a more solid and more respectable foundation, and to give full security to the subscribers against all future claims upon them.

IN THIS STAGE OF THE BUSINESS, and especially as a meeting of the subscribers is to be held in a few days for the purpose of determining what other steps shall be taken for carrying the proposed plan into execution, I have thought it to be my duty to lay all these particulars before the subscribers, and at the same time to state to them at length the general outline of the plan I have taken the liberty to propose, and in the execution of which, if it should be adopted, I am ready to take any part that the subscribers may wish me to take.

RUMFORD.

BROMPTON ROW, *4th March*, 1799.

PROPOSALS, &c.

The two great objects of the Institution being the speedy and general diffusion of the knowledge of all new and useful improvements, in whatever quarter of the world they may originate, and teaching the application of scientific discoveries to the improvement of arts and manufactures in this country, and to the increase of domestic comfort and convenience, these objects will constantly be had in view, not only in the arrangement and execution of the plan, but also in the future management of the Institution.

As much care will be taken to confine the establishment within its proper limits as to place it on a solid foundation, and to render it an ornament to the Capital and an honour to the British nation.

In the execution of the plan it is proposed to proceed in the following manner:—

A place having been fixed on by the Managers for forming the Institution,

Spacious and airy rooms will be prepared for the reception and public exhibition of all such new and mechanical inventions and im-

* See a list of these names, p. xxviii.

provements as shall be thought worthy of the public notice, and more especially of all such contrivances as shall tend to increase the conveniences and comforts of life, to promote domestic economy, to improve taste, or to promote useful industry.

The most perfect models of the full size will be provided and exhibited in different parts of this public repository of all such new mechanical inventions and improvements as are applicable to the common purposes of life. Under this head will be included:—

Cottage Fire-places, and Kitchen Utensils for Cottagers.
A complete Kitchen for a Farm-house, with all the necessary Utensils.
A complete Kitchen, with Kitchen Utensils, for the family of a gentleman of fortune.
A complete Laundry for a gentleman's family, or for a public hospital, including Boilers, Washing-room, Ironing-room, Drying-room, &c.
Several of the most approved German, Swedish, and Russian Stoves, for heating rooms and passages.

In order that those who visit this establishment may be enabled to acquire more just ideas of these various mechanical contrivances, and of the circumstances on which their *peculiar merit* principally depends, the machinery exhibited will, as far as it shall be possible, *be shown in action*, or in *actual use*, and with regard to many of the articles it is evident that this can be done without any difficulty, and with very little additional expense.

Open Chimney Fire-places on the most approved principles will be fitted up as models in the different rooms, and fires will be kept constantly burning in them during the cold season.
Ornamental, as well as economical Grates, for Open Chimney Fire-places, will also be exhibited; as also,
Ornamental Stoves, in the form of elegant Chimney-pieces, for halls, drawing-rooms, eating-rooms, &c.

It is likewise proposed to exhibit *working models*, on a reduced scale, of that most curious and most useful machine, the Steam Engine.

Of Brewer's Boilers, with improved Fire-places.
Of Distiller's Coppers, with improved Fire-places and improved Condensers.
Of large Boilers for the kitchens of hospitals, and of Ships' Coppers, with improved Fire-places.

Farther, it is proposed to exhibit, in the repository of the Institution:—

Models of Ventilators for supplying rooms and ships with fresh air.

Models of Hot-houses, with such improvements as can be made in their construction.

Models of Lime-kilns, on various constructions.

Models of Boilers, Steam-boilers, &c., for preparing food for cattle that are stall-fed.

Models of Cottages on various constructions.

Spinning-wheels and Looms, on various constructions, for the use of the poor, and adapted to their circumstances, together with such other machinery as may be useful in giving them employment at home.

Models of all such new-invented Machines and Implements as bid fair to be of use in Husbandry.

Models of Bridges, on various constructions; together with *models of all such other machines and useful instruments as the Managers of the Institution shall deem worthy of the public notice*, and proper to be publicly exhibited in the repository of the Institution.

It is proposed that each article exhibited should be accompanied with a detailed account or description of it, properly illustrated by correct drawings. The name of the maker and the place of his abode will also be mentioned in this account, together with the price at which he is willing to furnish the article to buyers.

In order to carry into effect the second object of the Institution, namely, TEACHING THE APPLICATION OF SCIENCE to the USEFUL PURPOSES OF LIFE,

A lecture-room will be fitted up for philosophical lectures and experiments, and a complete LABORATORY and PHILOSOPHICAL APPARATUS, with the necessary instruments, will be provided for making *chemical* and other *philosophical experiments*.

In fitting up this lecture-room (which will never be used for any other purpose than for giving lectures in Natural Philosophy and Philosophical Chemistry) convenient places will be provided and reserved for the subscribers, and care will be taken to warm and light the room properly, and provide for a sufficient supply of fresh air, so as to render it comfortable and salubrious.

In engaging Lecturers for the Institution, care will be taken by the Managers to invite none but men of the first eminence in science to officiate in that most important and most distinguished situation; and no subjects will ever be permitted to be discussed at these lectures but such as are strictly scientifical, and immediately connected with that particular branch of science publicly announced as the subject of the lecture. The Managers to be responsible for the strict observance of this regulation.

In case there should be places to spare in the lecture-room, persons not subscribers will, on the recommendation of a subscriber, and on paying a certain small sum to be determined by the Managers, be permitted to attend the public lectures, or any one or more of them.

Among the various branches of science that will occasionally be made the subjects of these public lectures may be reckoned the following, *viz.* These lectures will treat:—

Of Heat, and its application to the various purposes of life.
Of the Combustion of Inflammable Bodies, and the relative quantities of Heat producible by the different substances used as fuel.
Of the Management of Fire and the Economy of Fuel.
Of the Principles of the Warmth of Clothing.
Of the Effects of Heat, and of Cold, and of hot, and of cold winds, on the human body, in sickness and in health.
Of the Effects of breathing vitiated and confined air.
Of the Means that may be used to render Dwelling-houses comfortable and salubrious.
Of the Methods of procuring and preserving Ice in Summer; and of the best principles for constructing Ice-houses.
Of the Means of preserving Food in different seasons and in different climates.
Of the Means of cooling Liquors in hot weather, without the assistance of ice.
Of Vegetation, and of the specific nature of those effects that are produced by Manures; and of the Art of composing Manures, and adapting them to the different kinds of soil.
Of the nature of those changes that are produced on substances used as food in the various processes of cookery.
Of the nature of those changes which take place in the Digestion of Food.
Of the Chemical Principles of the process of Tanning Leather; and of the objects that must particularly be had in view in attempts to improve that most useful art.
Of the Chemical Principles of the art of making Soap; of the art of Bleaching; of the art of Dyeing; and in general of *all the Mechanical Arts*, as they apply to the various branches of manufacture.

Of the Funds of the Institution.

It is proposed to raise the money necessary for defraying the expense of forming this Institution, and also for the future expense of keeping it up, in the following manner:—

1st. By the sums subscribed by the original founders, and sole *Proprietors* of the Institution, at *fifty guineas each person*, to be but once paid.

2dly. By the sums contributed by those who shall subscribe *for life*, at *ten guineas* each person, to be but once paid.

3dly. By the sums contributed by the *Annual Subscribers*, at two guineas *per annum* for each person.

4thly. By the particular donations and legacies that may be expected to be made for the purpose of extending and improving so interesting and so useful an Institution; and,

Lastly. By the sums that shall be received at the door from strangers who shall visit the repository of the Institution, or who shall obtain leave to frequent the philosophical lectures.

Privileges of the Original Subscribers or Proprietors of the Institution.

1*mo.* These subscribers, who will *never be called upon for any further contributions* after the sum subscribed (fifty guineas) shall have been once paid, will be effectually secured against all future legal claims and demands upon them, on account of any debts the Managers of the Institution may contract, as a charter for the Institution will be applied for and obtained, for the express purpose of providing for that security, before any other step shall be taken for carrying this plan into execution, and before any part of the money subscribed will be demanded.

2*do.* Proprietors will not be deemed liable to serve, either as Managers or as Visitors, against their consent; and none will be considered as candidates for either of those offices, or will be entered on the lists as candidates, or be proposed as such, except it be those who shall have previously signified their willingness to serve in one of those offices in case of their being elected.

3*tio.* For the still greater security of the proprietors, as well as to found the Institution on a more solid basis, one-half of the sums subscribed by the original Subscribers and Proprietors of the Institution will be permanently vested in the public funds, or in the purchase of freehold property, and the annual produce thereof employed in defraying the expense of keeping up the Institution.

4*to.* Each original Subscriber and Proprietor of the Institution to be an hereditary Governor of the Institution—to have a perpetual *transferable* share in all the property belonging to it—to have a voice in the election of the Managers of the Institution; as also in the election of the Committee of Visitors—to have moreover two *transferable* tickets of perpetual admission into the establishment, and into every part of it; and two *transferable* tickets of admission to all the public philosophical lectures and experiments.

5*to.* Although the shares of Proprietors and all the privileges annexed to them are hereditary, and are also *transferable* by sale or by donation, yet those to whom such shares are conveyed by sale or by donation must, in order to their being rendered capable of holding them, have obtained the approbation and consent of the majority of the Managers for the time being. Those who shall become possessed of these shares by inheritance will not stand in need of the consent of the Managers to be qualified to hold them, and to enjoy the rights and privileges annexed to them.

6*to.* Proprietors' tickets will admit any persons who shall be the bearers of them.

7*mo.* Proprietors will have the privilege of recommending persons for admittance to the philosophical lectures and experiments, and the persons so recommended will be admitted in all cases where there shall be room for their accommodation, provided that the persons so admitted conform to the rules and regulations which will be established by the Managers for the preservation of order and decorum within the walls of the Institution.

8*vo.* No more than *forty per cent.* of the sum subscribed by each Proprietor will be wanted immediately, and the remainder may be furnished in three equal payments at the expiration of the three next succeeding half-years; but it will be in the option of Proprietors to pay the whole sum of fifty guineas at once if they should prefer doing it.

Privileges of the Subscribers for Life.

Each subscriber of this class will receive *one* ticket for life, but not transferable, of free admission into the Institution, and into every part of it, together with *one* other ticket for life, but not transferable, of free admission to all public philosophical lectures and experiments.

Privileges of Annual Subscribers.

Each Annual Subscriber will receive *one* ticket for one year, but not transferable, of admission into the Institution, and into every part of it; as also *one* ticket for one year, but not transferable, of admission to all the public philosophical lectures and experiments. Subscribers of this class will moreover have a right of becoming subscribers for life, on paying at any time within the year for which they subscribe an additional sum of eight guineas.

Privileges that are common to Subscribers of all Denominations.

1*mo.* Subscribers for life, and Annual Subscribers, as well as the Proprietors of the Institution, will be entitled to have copies or drawings (made at their own expense however) of any of the models in the repository, and this even when such copies are designed for the use of their friends, as well as when they are wanted for their own private use; and for their better and more speedy accommodation, workshops will be prepared, and workmen provided under the direction of the Managers, for executing such work properly, and at reasonable prices. And to prevent mistakes, all copies or drawings that shall be made of the machines, models, and plans lodged in the repository of the Institution, will be examined by persons appointed for that purpose, and marked with the seal or stamp of the Institution.

2*do.* Tradesmen and artificers employed in executing any work after any of the models lodged in the repository, will, on the recommendation of a Proprietor, or of a Subscriber for life, or for one year, be allowed free access to such model as often as shall be necessary; and any workman or artificer so recommended, who shall be willing to furnish to buyers any article exhibited in the repository that is in his line of business, will be allowed to place a specimen of such article of his manufacture in the repository, with his name and place of abode attached to it, together with the price at which he can furnish it, such specimen having been examined and approved by the Managers.

Of the Government and Management of the Institution.

1*mo.* All the affairs of the Institution will be directed and governed by *nine* Managers chosen by and from among the Proprietors of the Institution.

2*do.* For the greater convenience of the Proprietors, and to spare them the trouble of a General Meeting, all the elections of Managers, after the first, will be made by ballot, by means of sealed lists of names sent in by the Proprietors individually to the Institution, which lists will be opened, and the result of the election ascertained and published by the united Committees of the Managers and of the Visitors for the time being.

3*tio.* The first set of Managers will be chosen by the first fifty or more original subscribers, at a General Meeting of them to be held for that purpose; and of this first set of Managers three will be chosen to serve *three years*, three to serve *two years*, and three to serve *one year*, reckoned from the 25th day of March, 1799.

4*to.* All Managers, as well those of the first set as others, will be capable of being *re-elected* without limitation.

5*to.* The elections of Managers to be made annually on the 25th day of the month of March,* and fourteen days previous to each election the Managers for the time being will send to each Proprietor individually a printed list containing the names of all such of the Proprietors as shall have offered or consented to be candidates for the places among the Managers that are to be filled up. On this printed list which each Proprietor will receive he will indicate the persons to whom he gives his suffrage by making a mark with a pen and ink in the form of a small cross just before the names of those persons; and this being done, he will seal up the list without signing it, and send it to the Institution, directed "To the United Committees of the Managers and of the Visitors." In order that these lists may be recognized on their being returned to the Institution, they will all be marked with the stamp of the Institution, previous to their being issued or sent to the Proprietors. And for still further security, each Proprietor will

* If any other season should be thought more convenient for these elections, it will of course be chosen instead of that here proposed.

be requested to send in his or her sealed list of names under an additional cover, signed with his or her own name, which additional cover will be taken off, and all the sealed lists mixed together in an urn, previous to any of them being opened—an arrangement that will effectually prevent the vote of any individual subscriber being known.

6*to.* The Managers are to serve in that office without any pay or emolument, or pecuniary advantage whatever; and by their acceptance of their office they shall be deemed solemnly to pledge themselves to the Proprietors of the Institution and to the public for the faithful discharge of their duty as Managers, and also for their strict adherence to the fundamental principles of the government of the Institution as established at its formation.

7*mo.* The Managers are to take care that the property of the Institution, as far as it shall be practicable, be insured against accidents by fire.

8*vo.* The Managers will cause exact and detailed accounts to be kept of all the property belonging to the Institution, as also of all receipts and expenditures. They will also keep regular minutes of all their proceedings, and will take care to preserve the most exact order and the strictest economy in the management of all the affairs and concerns of the Institution.

9*mo.* The Managers are never, on any pretext, or in any manner whatever, to dispose of any money or property of any kind belonging to the Institution in *premiums*, as the design or object of the Institution is NOT TO GIVE REWARDS to the authors of ingenious inventions, but to *diffuse the knowledge of such improvements as bid fair to be of general use*, and to facilitate the general introduction of them; and to excite and assist the ingenious and the enterprising by *the diffusion of science*, and by awakening a spirit of inquiry.

10*mo.* The ordinary meetings of the Managers for the dispatch of the current business of the Institution will be held weekly, namely, on every , at the hour of ; and extraordinary meetings will be held as often as shall be found necessary.

11*mo.* Any three or more of the Managers being present at any ordinary or at an extraordinary meeting, the others having been duly summoned, to be a quorum.

12*mo.* The Managers will be authorized to make all such standing orders and regulations as they shall deem necessary to the preservation of order and decorum in the Institution, as also such regulations respecting the manner of transacting the business of the Institution as they shall think proper and convenient, or that may be necessary in order to regulate the responsibility of the Managers for their acts and deeds; all such standing orders and regulations must, however, in order to their being valid, be approved by six at least of the Managers, and they must all be published and made known to all the Proprietors.

Of the Committee of Visitors.

1*mo.* The Committee of Visitors will be composed of *nine* persons, the first set to be elected three months after the opening of the Institution.

2*do.* Three persons of the nine of which this Committee will consist will be chosen for *three years*, three of them to serve *two years*, and three of them to serve *one year*, reckoned from the 25th of March, 1799.

3*tio.* Any three or more of the members of this Committee being present at any meeting of the Committee, the others having been duly summoned, to make a quorum.

4*to.* It will be the business of this Committee formally to inspect and examine the Institution, and every part and detail of it, once every year, namely, on the 25th day of the month of March, and to give a printed account or report to the Proprietors, and to the subscribers of all denominations, of its state and condition, and of the degree and manner in which it is found to answer the important ends for which it was designed. This Committee will also once every year, namely, on the 25th of the month of March, examine and audit the accounts of the receipts and expenditures of the Institution kept by the Managers or by their orders; and the report of the Committee of Visitors on this audit will always make the first article in their public annual reports.

5*to.* A person actually serving as a Visitor will not be eligible as a Manager, nor can his name be put on the list of candidates for that office till one whole year shall have elapsed after he shall have ceased to belong to the Committee of Visitors. Those, however, who serve as Visitors will be capable of being *re-elected* on that Committee without limitation.

Miscellaneous Articles.

1*mo.* The Managers will take care to procure, and to exhibit in the repository, as early as possible, models of all such new and useful mechanical inventions and improvements as shall, from time to time, be made in this or in any other country.

2*do.* All presents to the Institution, and all new purchases and acquisitions of every kind, will be and remain the joint property of the Proprietors of the Institution, and of their heirs and assigns, and all the surplus of the income of the Institution, over and above what shall be found necessary for maintaining it and keeping it up, will be employed by the Managers in making additions to the local accommodations of the Institution, or in augmenting the collection of models, or in making additions to the philosophical apparatus, accordingly as the Managers of the Institution for the time being shall deem most useful.

3*tio.* In order that the Proprietors of the Institution, and the subscribers, may have the earliest notice of all new discoveries and useful

improvements that shall be made, from time to time, not only in this country, but also in all the different parts of the world, the Managers will employ the proper means for obtaining, as early as possible, from every part of the British Empire, and from all foreign countries, authentic accounts of all such new and interesting discoveries in the various branches of science, and in arts and manufactures, and also of all such new and useful mechanical improvements as shall be made; and a room will be set apart in the Institution where all such information will be lodged, and where it will be kept for the sole and exclusive use and inspection of the Proprietors and subscribers, and where no stranger will ever be admitted.

SUPPLEMENT.

Since the foregoing sheets were printed off and distributed among the original subscribers, a meeting of the subscribers has been held, when the following Resolutions were unanimously taken:—

"INSTITUTION

for diffusing the Knowledge, and facilitating the general Introduction of useful Mechanical Inventions and Improvements; *and for teaching, by Courses of* Philosophical Lectures and Experiments, *the Application of Science to the common Purposes of Life.*

"At a General Meeting of the Proprietors, held at the house of the Right Honourable Sir Joseph Banks, Bart., K.B., in Soho Square, on the 7th day of March, 1799,

"The Right Hon. SIR JOSEPH BANKS in the Chair,

"the following list of the Proprietors, and original subscribers of fifty guineas each, was read:—

Sir Robert Ainslie, Bart.
J. J. Angerstein, Esq.
Right Hon. Sir Joseph Banks, K.B.
Thomas Bernard, Esq.
Scrope Bernard, Esq., M.P.
The Earl of Besborough.
Rowland Burdon, Esq., M.P.
James Burton, Esq.
Timothy Brent, Esq.
Henry Cavendish, Esq.
Rich. Clark, Esq., Chamb. of London.
Sir John Colpoys, K.B.
John Craufurd, Esq.
The Duke of Devonshire, K.G.
Andrew Douglas, Esq.
The Lord Bishop of Durham.
The Earl of Egremont.
George Ellis, Esq., M.P.
Joseph Grote, Esq.
Sir Robert Bateson Harvey, Bart.
Sir John Cox Hippesley, Bart.
Henry Hoare, Esq.
Lord Hobart.
Lord Holland.
Henry Hope, Esq.
Thomas Hope, Esq.
Lord Keith, K.B.
William Lushington, Esq., M.P.
Sir John Macpherson, Bart., M.P.
William Manning, Esq., M.P.
The Earl of Mansfield.
The Earl of Morton, K.T.
Lord Ossulston.
Thomas Palmer, Esq.

The Lord Viscount Palmerston, M.P.
Edward Parry, Esq.
Right Hon. Thomas Pelham, M.P.
John Penn, Esq.
William Morton Pitt, Esq., M.P.
Sir James Pulteney, Bart., M.P.
Sir John Buchanan Riddell, Bart.
Count Rumford.
Sir John Sinclair, Bart., M.P.
Lord Somerville.
John Spalding, Esq., M.P.
The Earl Spencer, K.G.
Sir George Staunton, Bart.
John Sullivan, Esq.
Richard Joseph Sulivan, Esq.
Lord Teignmouth.
John Thomson, Esq.
Samuel Thornton, Esq., M.P.
Henry Thornton, Esq., M.P.
George Vansittart, Esq., M.P.
William Wilberforce, Esq., M.P.
The Earl of Winchelsea.
Hon. James Stuart Wortley, M.P.
Sir William Young, Bart., M.P.

"*The following Resolutions were agreed to unanimously:*—

"I.—That before any measures are taken for carrying the plan into execution, a petition be presented to His Majesty, praying that he would be graciously pleased to grant a Charter to the Institution.

"II.—That an outline of the plan be laid before the Right Honourable Mr. Pitt and His Grace the Duke of Portland.

"III.—That, for these purposes, it is expedient to elect the Committee of Managers.

"IV.—That the following Proprietors (*who have agreed to serve in case they shall be elected*) be now elected as the *first Managers* of the Institution:—

For three years.

The Earl Spencer.
Count Rumford.
Richard Clark, Esq.

For two years.

The Earl of Egremont.
Rt. Hon. Sir Joseph Banks.
Rich. Joseph Sulivan, Esq.

For one year.

The Earl of Morton.
The Rt. Hon. Thomas Pelham.
Thomas Bernard, Esq.

"V.—That the said Managers be desired to solicit a Charter for the Institution, upon principles conformable to the Proposals which have been printed and distributed, and (as soon as the Charter is obtained) to publish the plan for the benefit of the public, in such manner as they shall deem most expedient; and also to take preparatory measures for opening the Institution.

"That these Resolutions be inserted in the public papers.

"Jos. Banks, *Chairman.*"

Sir Joseph Banks having quitted the Chair,

"RESOLVED,

"That the thanks of the Meeting be given to him for his conduct in the Chair."

"N.B.—Count Rumford's original Proposals for forming the Institution may be had of Messrs. Cadell and Davies, in the Strand."

Since this Meeting of the PROPRIETORS, a Meeting of the MANAGERS has been held, and the following Resolutions taken:—

"At the first meeting of the MANAGERS of the INSTITUTION, held at the house of the Right Honourable Sir Joseph Banks, in Soho Square, the 9th of March, 1799:

"On a motion made by COUNT RUMFORD,

"I.—RESOLVED, That SIR JOSEPH BANKS be requested to take the Chair; and that he do continue to preside at all future Meetings of the Managers, until a Charter shall have been obtained from HIS MAJESTY for the Institution.

"II.—RESOLVED, That all Acts and Deeds of the Managers, in carrying on the business of the Institution, be transacted and done in the name of '*The* MANAGERS *of the* INSTITUTION.'

"III.—RESOLVED, That at each Meeting of the Managers, one of the Managers present be elected by a majority of those present, to act as SECRETARY to the Managers at that Meeting.

"IV.—RESOLVED, That the Minutes of the Proceedings of each Meeting of the Managers for the dispatch of the business of the Institution, as well as all Orders, Resolutions, and other Acts and Deeds of the Managers, be signed by the person who acts as President, and also by the person who acts as Secretary at the Meeting at which such business is transacted.

"V.—RESOLVED, That the persons present at this Meeting do now proceed to make choice of one of their number to act as Secretary at the present Meeting.

"VI.—RESOLVED, That THOMAS BERNARD, Esq., is duly elected to act as Secretary at the present Meeting.

"VII.—RESOLVED, That the Proposals for forming the Institution, as published by COUNT RUMFORD, be approved and adopted by the Managers, subject, however, to such partial modifications as shall be by them found to be necessary or useful.

"VIII.—RESOLVED, That the EARL of MORTON, the EARL SPENCER, SIR JOSEPH BANKS, and MR. PELHAM, or any one or more of them, be

requested to lay the Proposals for forming the Institution before HIS MAJESTY and the ROYAL FAMILY, and before HIS MAJESTY'S MINISTERS and the GREAT OFFICERS OF STATE.

"IX.—RESOLVED, That the Proposals for forming the Institution be laid before the MEMBERS of BOTH HOUSES OF PARLIAMENT, and also before the members of HIS MAJESTY'S MOST HONOURABLE PRIVY COUNCIL, and the TWELVE JUDGES.

"Messrs. Cadell and Davies, booksellers in the Strand, having generously offered to make a donation to the Institution of 500 copies of the original Proposals for forming the Institution, published by Count Rumford:

"X.—RESOLVED, That the thanks of the Managers be given to Messrs. Cadell and Davies for this donation; that it be accepted; and that these 500 copies of the Proposals be distributed among such persons as the Managers may think most likely to give their assistance in forming the Institution."

Although the author of the foregoing Proposals is anxious to avoid every appearance of taking a liberty with his readers which he is very sensible he has no right to take, and which would be improper on many accounts,—that of soliciting as a favour their countenance and support in carrying into execution the plan he has had the honour to lay before them, yet as it is possible that some of those who may read these Proposals may be disposed to give that assistance in some one or more of the various ways in which it can be given and received, to save trouble to those who may be so disposed, the two following leaves, which, when taken out of this pamphlet, will form an open letter, are annexed to this publication, which paper being divided into separate columns, distinguished according to the different heads under which the subscriptions can be regularly entered, those who are disposed to contribute to the execution of the plan are requested to put down their names and places of abode in the column they may choose, and after sealing up the paper with a wafer, send it according to its address.

Those who are desirous of becoming Proprietors of the Institution are requested to consider themselves as candidates for Proprietors' places until they shall have been elected as such by a majority of the Managers.

Those who put down their names in the lists as *Subscribers for life*, or as *Annual Subscribers*, will not be called upon for the sums subscribed till after the Institution shall have been opened.

Those who make *donations* to the Institution are requested to fix the time or periods when the sums proposed to be given may be called for by the Managers.

To

The Right Honourable Sir Joseph Banks, Bart., K.B.

Soho Square.

NAMES AND PLACES OF ABODE OF PERSONS WHO ARE WILLING TO CONTRIBUTE TOWARDS FORMING AND MAINTAINING A PUBLIC INSTITUTION FOR DIFFUSING THE KNOWLEDGE AND FACILITATING THE GENERAL INTRODUCTION OF USEFUL MECHANICAL INVENTIONS AND IMPROVEMENTS, &C.

Candidates for Proprietors' Shares at 50 Guineas each.	Subscribers for Life at 10 Guineas each.	Annual Subscribers at 2 Guineas each.

Those who are desirous of making DONATIONS to the Institution are requested to put down their Names and Places of Abode, together with the sums they are willing to give, on the opposite side of this leaf.

Royal Institution of Great Britain.

1870.

WEEKLY EVENING MEETING,

Friday, January 21, 1870.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

JOHN TYNDALL, Esq. LL.D. F.R.S.

PROFESSOR OF NATURAL PHILOSOPHY IN THE ROYAL INSTITUTION.

On Dust and Disease.

SOLAR light in passing through a dark room reveals its track by illuminating the dust floating in the air. "The sun," says Daniel Culverwell, "discovers atomes, though they be invisible by candle-light, and makes them dance naked in his beams." *

In my researches on the decomposition of vapours by light I was compelled to remove these "atomes" and this dust. It was essential that the space containing the vapours should embrace no visible thing; that no substance capable of scattering the light in the slightest sensible degree should, at the outset of an experiment, be found in the "experimental tube" traversed by the luminous beam.

For a long time I was troubled by the appearance there of floating dust, which though invisible in diffuse daylight was at once revealed by a powerfully condensed beam. Two tubes were placed in succession in the path of the dust: the one containing fragments of glass wetted with concentrated sulphuric acid; the other, fragments of marble wetted with a strong solution of caustic potash. To my astonishment it passed through both. The air of the Royal Institution sent through these tubes at a rate sufficiently slow to dry it, and to remove its carbonic acid, carried into the experimental tube a considerable amount of mechanically suspended matter, which was illuminated when the beam passed through the tube. The effect was substantially the same when the air was permitted to bubble through the liquid acid and through the solution of potash. The core of the bubble does not touch the acid, and even the dust particles which come into contact with the acid require time to be wetted by it. When left sufficiently long in contact with the acid, the particles are destroyed.

Thus, on the 5th of October, 1868, successive charges of air were admitted through the potash and sulphuric acid into the exhausted

* On a day of transient shadows there is something almost magical in the rise and dissolution of the luminous beams among the scaffolding poles of the Royal Albert Hall.

experimental tube. Prior to the admission of the air the tube was *optically empty;* it contained nothing competent to scatter the light. After the air had entered the tube, the conical track of the electric beam was in all cases clearly revealed. This indeed was a daily observation at the time to which I now refer.

I tried to intercept this floating matter in various ways; and on the day just mentioned, prior to sending the air through the drying apparatus, I carefully permitted it to pass over the tip of a spirit-lamp flame. The floating matter no longer appeared, having been burnt up by the flame. It was therefore of *organic origin*. I was by no means prepared for this result; for I had thought that the dust of our air was, in great part, inorganic and non-combustible.

Mr. Valentin had the kindness to procure for me a small gas-furnace containing a platinum tube, which could be heated to vivid redness.* The tube also contained a roll of platinum gauze, which, while it permitted the air to pass through it, ensured the practical contact of the dust with the incandescent metal. The air of the laboratory was permitted to enter the experimental tube, sometimes through the cold, and sometimes through the heated, tube of platinum. The rapidity of admission was also varied. In the first column of the following Table the quantity of air operated on is expressed by the number of inches which the mercury gauge of the air-pump sank when the air entered. In the second column the condition of the platinum tube is mentioned, and in the third the state of the air which entered the experimental tube.

Quantity of Air.	State of Platinum Tube.	State of Experimental Tube.
15 inches	Cold	Full of particles.
15 „	Red-hot	Optically empty.

The phrase "optically empty" shows that when the conditions of perfect combustion were present, the floating matter totally disappeared. It was wholly burnt up, leaving no sensible residue. The experiment was repeated many times with the same invariable result.

The particles floating in the air of London being thus proved to be of organic origin,† I sought to burn them up at the focus of a concave reflector. One of the powerfully convergent mirrors employed in my experiments on combustion by dark rays was here made use of, but I failed in the attempt. Doubtless the floating particles are in part

* Pasteur was, I believe, the first to employ such a tube.

† According to an analysis kindly furnished to me by Dr. Percy, the dust collected *from the walls* of the British Museum contains fully 50 per cent. of inorganic matter. I have every confidence in the results of this distinguished chemist; they show that the *floating* dust of our rooms is, as it were, winnowed from the heavier matter. As bearing directly upon this point I may quote the following passage from Pasteur:—"Mais ici se présente une remarque: la poussière que l'on trouve à la surface de tous les corps est soumise constamment à des courants d'air, qui doivent soulever ses particules les plus légères, au nombre desquelles se trouvent, sans doute, de préférence les corpuscules organisés, œufs ou spores, moins lourds généralement que les particules minérales."

transparent to radiant heat, and are so far incombustible by such heat Their rapid motion through the focus also aids their escape. They do not linger there sufficiently long to be consumed. A flame it was evident would burn them up, but I thought the presence of the flame would mask its own action among the particles.

In a cylindrical beam, which powerfully illuminated the dust of the laboratory, was placed an ignited spirit-lamp. Mingling with the flame, and round its rim, were seen wreaths of darkness resembling an intensely black smoke. On lowering the flame below the beam the same dark masses stormed upwards. They were at times blacker than the blackest smoke that I have ever seen issuing from the funnel of a steamer; and their resemblance to smoke was so perfect as to lead the most practised observer to conclude that the apparently pure flame of the alcohol lamp required but a beam of sufficient intensity to reveal its clouds of liberated carbon.

But is the blackness smoke? This question presented itself in a moment. A red-hot poker was placed underneath the beam, and from it the black wreaths also ascended. A large hydrogen flame was next employed, and it produced those whirling masses of darkness far more copiously than either the spirit-flame or poker. Smoke was therefore out of the question.

What then was the blackness? It was simply that of stellar space; that is to say, blackness resulting from the absence from the track of the beam of all matter competent to scatter its light. When the flame was placed below the beam the floating matter was destroyed *in situ*; and the air, freed from this matter, rose into the beam, jostled aside the illuminated particles, and substituted for their light the darkness due to its own perfect transparency. Nothing could more forcibly illustrate the invisibility of the agent which renders all things visible. The beam crossed, unseen, the black chasm formed by the transparent air, while at both sides of the gap the thick-strewn particles shone out like a luminous solid under the powerful illumination.

But here a difficulty meets us. It is not necessary to burn the particles to produce a stream of darkness. Without actual combustion, currents may be generated which shall exclude the floating matter, and therefore appear dark amid the surrounding brightness. I noticed this effect first on placing a red-hot copper ball below the beam, and permitting it to remain there until its temperature had fallen below that of boiling water. The dark currents, though much enfeebled, were still produced. They may also be produced by a flask filled with hot water.

To study this effect a platinum wire was stretched across the beam, the two ends of the wire being connected with the two poles of a voltaic battery. To regulate the strength of the current a rheostat was placed in the circuit. Beginning with a feeble current the temperature of the wire was gradually augmented; but before it reached the heat of ignition, a flat stream of air rose from it, which when looked at edgeways appeared darker and sharper than one of

the blackest lines of Fraunhofer in the solar spectrum. Right and left of this dark vertical band the floating matter rose upwards, bounding definitely the non-luminous stream of air. What is the explanation? Simply this. The hot wire rarefied the air in contact with it, but it did not equally lighten the floating matter. The convection current of pure air therefore passed upwards *among the inert particles*, dragging them after it right and left, but forming between them an impassable black partition. This elementary experiment enables us to render an account of the dark currents produced by bodies at a temperature below that of combustion.

When the wire is white hot, it sends up a band of intense darkness. This, I say, is due to the *destruction* of the floating matter. But even when its temperature does not exceed that of boiling water the wire produces a dark ascending current. This, I say, is due to the *distribution* of the floating matter. Imagine the wire clasped by the mote-filled air. My idea is that it heats the air and lightens it, without in the same degree lightening the floating matter. The tendency, therefore, is to start a current of clean air through the mote-filled air. Figure the motion of the air all round the wire. Looking at its transverse section we should see the air at the bottom of the wire bending round it right and left in two branch currents, ascending its sides and turning to fill the partial vacuum created above the wire. Now as each new supply of air filled with its motes comes in contact with the hot wire, the clean air, as just stated, is first started through the inert motes. They are dragged after it, but there is a fringe of cleansed air in advance of the motes. The two purified fringes of the two branch currents unite above the wire, and, keeping the motes that once belonged to them right and left, they form by their union the dark band observed in the experiment. This process is incessant. Always the moment the mote-filled air touches the wire this distribution is effected, a permanent dark band being thus produced. Could the air and the particles under the wire pass *through* its mass we should have a vertical current of particles, but no dark band. For here, though the motes would be left behind at starting, they would hotly follow the ascending current and thus abolish the darkness.

It has been said that when the platinum wire is intensely heated, the floating matter is not only distributed, but destroyed. Let this be proved. I stretched a wire about 4 inches long through the air of an ordinary glass shade resting on its stand. Its lower rim rested on cotton wool, which also surrounded the rim. The wire was raised to a white heat by an electric current. The air expanded, and some of it was forced through the cotton wool, while when the current was interrupted and the air within the shade cooled, the expelled air in its return did not carry motes along with it. At the beginning of this experiment the shade was charged with floating matter; at the end of half an hour it was optically empty.

On the wooden base of a cubical glass shade measuring 11½ inches

a side, upright supports were fixed, and from one support to the other 38 inches of platinum wire were stretched in four parallel lines. The ends of the platinum wire were soldered to two stout copper wires which passed through the base of the shade and could be connected with a battery. As in the last experiment the shade rested upon cotton wool. A beam sent through the shade revealed the suspended matter. The platinum wire was then raised to whiteness. In five minutes there was a sensible diminution of the matter, and in ten minutes it was totally consumed. This proves that when the platinum wire is sufficiently heated, the floating matter, instead of being distributed, is destroyed.

But is not the matter really of a character which permits of its destruction by the moderately heated platinum wire also? Here is the reply:—

1. A platinum tube with its plug of platinum gauze was connected with an experimental tube, through which a powerful beam could be sent from an electric lamp placed at its end. The platinum tube was heated till it glowed feebly but distinctly in the dark. The experimental tube was exhausted and then filled with air which had passed through the red-hot tube. A considerable amount of floating matter which had escaped combustion was revealed by the electric beam.

2. The tube was raised to brighter redness and the air permitted to pass slowly through it. Though diminished in quantity, a certain amount of floating matter passed into the exhausted experimental tube.

3. The platinum tube was rendered still hotter; a barely perceptible trace of the floating matter now passed through it.

4. The experiment was repeated, with the difference that the air was sent more slowly through the red-hot tube. The floating matter was totally destroyed.

5. The platinum tube was now lowered until it bordered upon a visible red heat. The air sent through it still more slowly than in the last experiment carried with it a cloud of floating matter.

If then the suspended matter is destroyed by a bright red heat, much more is it destroyed by a flame whose temperature is vastly higher than any here employed. So that the blackness introduced into a luminous beam where a flame is placed beneath it is due, as stated, to the destruction of the suspended matter. At a dull red heat, however, and still more when only on the verge of redness, the platinum tube permitted the motes to pass freely. In the latter case the temperature was 800° or 900° Fahr. This was unable to destroy the suspended matter; much less, therefore, would a platinum wire heated to 212° be competent to do so. Such a wire can only distribute the matter, not destroy it.

The floating dust is revealed by intense local illumination. It is seen by contrast with the adjacent illuminated space; the brighter the illumination the more sensible is the difference. Now the beam employed in the foregoing experiments is not of the same brightness throughout its entire transverse section. Pass a white switch, or an

ivory paper-cutter, rapidly across the beam, the impression of its section will linger on the retina. The section seems to float for a moment in the air as a luminous circle with a rim much brighter than its central portion. The core of the beam is thus seen to be enclosed by an intensely luminous sheath. An effect complementary to this is observed when the beam is intersected by the dark band from the platinum wire. The brighter the illumination, the greater must be the relative darkness consequent on the withdrawal of the light. Hence the cross section of the sheath surrounds the dark band as a darker ring.

Oxygen, hydrogen, nitrogen, carbonic acid, so prepared as to exclude all floating particles, produce the darkness when poured or blown into the beam. Coal-gas does the same. An ordinary glass shade placed in the air with its mouth downwards permits the track of the beam to be seen crossing it. Let coal-gas or hydrogen enter the shade by a tube reaching to its top, the gas gradually fills the shade from the top downwards. As soon as it occupies the space crossed by the beam, the luminous track is instantly abolished. Lifting the shade so as to bring the common boundary of gas and air above the beam, the track flashes forth. After the shade is full, if it be inverted, the gas passes upwards like a black smoke among the illuminated particles.

The air of our London rooms is loaded with this organic dust, nor is the country air free from its pollution. However ordinary daylight may permit it to disguise itself, a sufficiently powerful beam causes the air in which the dust is suspended to appear as a semi-solid rather than as a gas. Nobody could, in the first instance, without repugnance place the mouth at the illuminated focus of the electric beam and inhale the dirt revealed there. Nor is the disgust abolished by the reflection that, although we do not see the nastiness, we are churning it in our lungs every hour and minute of our lives. There is no respite to this contact with dirt; and the wonder is, not that we should from time to time suffer from its presence, but that so small a portion of it would appear to be deadly to man.

And what is this portion? It was some time ago the current belief that epidemic diseases generally were propagated by a kind of malaria, which consisted of organic matter in a state of *motor-decay*; that when such matter was taken into the body through the lungs or skin, it had the power of spreading there the destroying process which had attacked itself. Such a spreading power was visibly exerted in the case of yeast. A little leaven was seen to leaven the whole lump, a mere speck of matter in this supposed state of decomposition being apparently competent to propagate indefinitely its own decay. Why should not a bit of rotten malaria work in a similar manner within the human frame? In 1836 a very wonderful reply was given to this question. In that year Cagniard de la Tour discovered the *yeast plant*, a living organism, which when placed in a proper medium feeds, grows, and reproduces itself, and in this way carries on the process which we name fermentation. Fermentation was thus proved to be a product of life instead of a process of decay.

Schwann, of Berlin, discovered the yeast plant independently; and in February, 1837, he also announced the important result, that when a decoction of meat is effectually screened from ordinary air, and supplied solely with calcined air, putrefaction never sets in. Putrefaction, therefore, he affirmed to be caused by something derived from the air, which something could be destroyed by a sufficiently high temperature. The experiments of Schwann were repeated and confirmed by Helmholtz, Ure, and Pasteur. But as regards fermentation, the minds of chemists, influenced probably by the great authority of Gay-Lussac, who ascribed putrefaction to the action of oxygen, fell back upon the old notion of matter in a state of decay. It was not the living yeast plant, but the dead or dying parts of it, which, assailed by oxygen, produced the fermentation. This notion was finally exploded by Pasteur. He proved that the so-called "ferments" are not such; that the true ferments are organized beings which find in the reputed ferments their necessary food.

Side by side with these researches and discoveries, and fortified by them and others, has run the *germ theory* of epidemic disease.* The notion was expressed by Kircher, and favoured by Linnæus, that epidemic diseases are due to germs which float in the atmosphere, enter the body, and produce disturbance by the development within the body of parasitic life. While it was still struggling against great odds, this theory found an expounder and a defender in the President of this Institution. At a time when most of his medical brethren considered it a wild dream, Sir Henry Holland contended that some form of the germ theory was probably true. The strength of this theory consists in the perfect parallelism of the phenomena of contagious disease with those of life. As a planted acorn gives birth to an oak competent to produce a whole crop of acorns, each gifted with the power of reproducing its parent tree; and as thus from a single seedling a whole forest may spring: so, it is urged, these epidemic diseases literally plant their seeds, grow, and shake abroad new germs, which, meeting in the human body their proper food and temperature, finally take possession of whole populations. Thus Asiatic cholera, beginning in a small way in the Delta of the Ganges, contrived in seventeen years to spread itself over nearly the whole habitable world. The development from an infinitesimal speck of the virus of small-pox of a crop of pustules, each charged with the original poison, is another illustration. The reappearance of the scourge, as in the case of the 'Dreadnought' at Greenwich, reported on so ably by Dr. Budd and Mr. Busk, receives a satisfactory explanation from the theory which ascribes it to the lingering of germs about the infected place.

Surgeons have long known the danger of permitting air to enter an opened abscess. To prevent its entrance they employ a tube

* Nobody is likely to infer from this language that the speaker lays any claim to the authorship of the germ theory.

called a cannula, to which is attached a sharp steel point called a trocar. They puncture with the steel point, and by gentle pressure they force the pus through the cannula. It is necessary to be very careful in cleansing the instrument; and it is difficult to see how it can be cleansed by ordinary methods in air loaded with organic impurities, as we have proved our air to be. The instrument ought, in fact, to be made as hot as its temper will bear. But this is not done; and hence, notwithstanding all the surgeon's care, inflammation often sets in after the first operation, rendering necessary a second and a third. Rapid putrefaction is found to accompany this new inflammation. The pus, moreover, which was sweet at first, and showed no trace of animal life, is now fetid, and swarming with active little organisms called vibrios. Professor Lister, from whose recent able lecture this fact is derived, contends, with the strongest show of reason, that this rapid putrefaction and this astounding development of animal life are due to the entry of germs into the abscess during the first operation, and their subsequent nurture and development under favourable conditions of food and temperature. The celebrated physiologist and physicist Helmholtz is attacked annually by hay-fever. From the 20th of May to the end of June he suffers from a catarrh of the upper air-passages; and he has found during this period, and at no other, that his nasal secretions are peopled by these vibrios. They appear to nestle by preference in the cavities and recesses of the nose, for a strong sneeze is necessary to dislodge them.

These statements sound uncomfortable; but by disclosing our enemy they enable us to fight him. When he clearly eyes his quarry the eagle's strength is doubled, and his swoop is rendered sure. If the germ theory be proved true, it will give a definiteness to our efforts to stamp out disease which they could not previously possess. And it is only by definite effort under its guidance that its truth or falsehood can be established. It is difficult for an outsider like myself to read without sympathetic emotion such papers as those of Dr. Budd, of Bristol, on Cholera, Scarlet-fever, and Small-pox. He is a man of strong imagination, and may occasionally take a flight beyond his facts; but without this dynamic heat of heart the stolid inertia of the free-born Briton cannot be overcome. And as long as the heat is employed to warm up the truth without singeing it over much; as long as this enthusiasm can overmatch its mistakes by unequivocal examples of success, so long am I disposed to give it a fair field to work in, and to wish it God speed.

But let us return to our dust. It is needless to remark that it cannot be blown away by an ordinary bellows; or, more correctly, the place of the particles blown away is in this case supplied by others ejected from the bellows, so that the track of the beam remains unimpaired. But if the nozzle of a good bellows be filled with cotton wool not too tightly packed, the air urged through the wool is filtered of its floating matter, and it then forms a clean band of darkness in the illuminated dust. This was the filter used by Schroeder

in his experiments on spontaneous generation, and turned subsequently to account in the excellent researches of Pasteur. Since 1868 I have constantly employed it myself.

But by far the most interesting and important illustration of this filtering process is furnished by the human breath. I fill my lungs with ordinary air and breathe through a glass tube across the electric beam. The condensation of the aqueous vapour of the breath is shown by the formation of a luminous white cloud of delicate texture. It is necessary to abolish this cloud, and this may be done by drying the breath previous to its entering into the beam; or, still more simply, by warming the glass tube. When this is done the luminous track of the beam is for a time uninterrupted. The breath impresses upon the floating matter a transverse motion, the dust from the lungs making good the particles displaced. But after some time an obscure disc appears upon the beam, the darkness of which increases, until finally, towards the end of the expiration, the beam is, as it were, pierced by an intensely black hole, in which no particles whatever can be discerned. The air, in fact, has so lodged its dirt within the passage to the lungs as to render the last portions of the expired breath absolutely free from suspended matter. This experiment may be repeated any number of times with the same result. It renders the distribution of the dirt within the air-passages as manifest as if the chest were transparent.

I now empty my lungs as perfectly as possible, and placing a handful of cotton wool against my mouth and nostrils, inhale through it. There is no difficulty in thus filling the lungs with air. On expiring this air through the glass tube, its freedom from floating matter is at once manifest. From the very beginning of the act of expiration the beam is pierced by a black aperture. The first puff from the lungs abolishes the illuminated dust and puts a patch of darkness in its place; and the darkness continues throughout the entire course of the expiration. When the tube is placed below the beam and moved to and fro, the same smoke-like appearance as that obtained with a flame is observed. In short, the cotton wool, when used in sufficient quantity, completely intercepts the floating matter on its way to the lungs.

The application of these experiments is obvious. If a physician wishes to hold back from the lungs of his patient, or from his own, the germs by which contagious disease is said to be propagated, he will employ a cotton wool respirator. After the revelations of this evening such respirators must, I think, come into general use as a defence against contagion. In the crowded dwellings of the London poor, where the isolation of the sick is difficult, if not impossible, the noxious air around the patient may, by this simple means, be restored to practical purity. Thus filtered, attendants may breathe the air unharmed. In all probability the protection of the lungs will be the protection of the entire system. For it is exceedingly probable that the germs which lodge in the air-passages, and which, at their leisure, can work their

way across the mucous membrane, are those which sow in the body epidemic disease. If this be so, then disease can certainly be warded off by filters of cotton wool. I should be most willing to test their efficacy in my own person. And time will decide whether in lung diseases also the woollen respirator cannot abate irritation, if not arrest decay. M. Pasteur, for whose work in connection with this subject I entertain a very high admiration, has shown that the germs diminish as we ascend a mountain. By means of a cotton-wool respirator, so far as the germs are concerned, the air of the highest Alps may be brought into the chamber of the invalid. Fifty different occupations might be named in which irritation of the lungs and injured health arise from the inhalation of dust. A properly constructed air-filter of cotton wool would entirely abolish the evil. Such a filter, properly constructed, would also be found effectual in warming the air. Provision ought to be made for the frequent removal of the cotton, the cost of which is practically *nil*.

The question of "Spontaneous generation" is intimately connected with our present subject. On this point a kind of polar antagonism has long existed between different classes of investigators. Van Helmont gave a receipt for the manufacture of mice, and it was for ages firmly believed that the maggots in putrefying flesh were spontaneously produced. Redi, a member of the famous Academy del Cimento, destroyed this notion by proving that it was only necessary to protect the meat by a covering of gauze to prevent the reputed generation. In 1745 two very able men, Needham and Spallanzani, took opposite sides in the discussion, the former affirming and the latter denying the fact of spontaneous generation. At the beginning of our own century, we find on the affirmative side Lamarck, Oken, and J. Müller; and on the negative Schwann, Schultze, and Ehrenberg. The chief representatives of the two opposing parties in our day are Pouchet and Pasteur.

The method of inquiry pursued in this discourse will, I think, help to clear the field of discussion. The experimenters do not seem to have been by any means fully aware of the character of the atmosphere in which they worked; for if this had been the case, some of the experiments recorded would never have been made. For example, to make the destruction of atmospheric germs doubly sure, M. Pouchet, the distinguished supporter of the doctrine of spontaneous generation, burnt hydrogen in air and collected the water produced by the combustion. Even in this water he afterwards found organisms. But supposing he had seen, as you have, the manner in which the air is clouded with floating matter, would he have concluded that the deportment of water which had been permitted to trickle through such air could have the least influence in deciding this great question? I think not. Here is a quantity of water produced and collected exactly as M. Pouchet produced and collected his. This water is perfectly clear in the common light; but in the condensed electric beam it is seen to be laden with particles, so thick-strewn and minute,

as to produce a continuous cone of light. In passing through the air the water loaded itself with this matter, and hence became charged with incipient life.*

Let me now draw your attention to an experiment of Pasteur, which I believe perplexes some of the readers and admirers of that excellent investigator. Pasteur prepared twenty-one flasks, each containing a decoction of yeast, filtered and clear. He boiled the decoction, so as to destroy whatever germs it might contain, and while the space above the liquid was filled with pure steam he sealed his flasks with a blow-pipe. He opened ten of them in the deep, damp caves of the Paris Observatory, and eleven of them in the courtyard of the establishment. Of the former, one only showed signs of life subsequently. In nine out of the ten flasks no organisms of any kind were developed. In all the others organisms speedily appeared.

Now here is an experiment conducted in Paris, which shows that the air of one locality can develop life when the air of another locality cannot. Let us see whether we cannot here in London justify and throw light upon this experiment. I place this large flask in the beam, and you see the luminous track crossing it from side to side. The flask is filled with the air of this room, charged with its germs and its dust, and hence capable of illumination. But here is another similar flask, which cuts a clear gap out of the beam. It is filled with unfiltered air, and still no trace of the beam is visible. Why? By pure accident I stumbled on this flask in our apparatus room, and on inquiry learnt that it had been a short time previously taken out of one of the cellars below stairs. Other flasks were in the same cellar. I had three of them brought up to me; they were optically empty. The still air had deposited its dust, germs and all, and was itself practically free from suspended matter. You can now understand the impotence of the air of the Paris caves. The observation illustrates at once the influence of the germs and the accuracy of Pasteur.

The air of the cellar was afterwards examined by the electric lamp. Though less heavily charged than the air outside, it was by no means free from particles. This was to be expected, because the door of the cellar was frequently opened. The flasks themselves were the true tranquil chambers; on their sides the dust had been deposited, and to them it firmly clung. To prove this several flasks about ten inches in diameter were filled with common air, corked, and laid upon a table in the laboratory. After two days' quiet they were optically empty.

Nor is it necessary even to cork the flasks; for with their mouths open the air within them is scarcely disturbed, certainly not displaced. Two days' rest on one of the laboratory tables suffices to deposit the organic dust and to render the open flasks optically empty.

I have had a chamber erected with a view to experiments on this

* In this case a polished silver basin was soldered to one end of a wide brass tube; the tube was filled with ice, the hydrogen flame was permitted to play upon the basin, and the water of condensation was then collected. Dr. Child also objects to Pouchet's experiment.

subject. The lower half is of wood, its upper half being enclosed by four glazed window-frames. The chamber tapers to a truncated cone at the top. It measures in plan 3 ft. by 2 ft. 6 in., and its height is 5 ft. 10 in. On the 6th of February this chamber was closed, and every crevice that could admit dust or cause displacement of the air was carefully pasted over with paper. The electric beam at first revealed the floating dust within the chamber as it did the air of the laboratory. The chamber was examined almost daily; a perceptible diminution of the floating matter being noticed on each occasion. At the end of a week the chamber was optically empty, exhibiting no trace of matter competent to scatter the light. But where the beam entered, and where it quitted the chamber, the white circles stamped upon the interior surfaces of the glass showed what had become of the dust. It clung to those surfaces, and from them instead of from the air, the light was scattered. If the electric beam were sent through the air of the Paris caves, the cause of its impotence as a generator of life would, I venture to predict, be revealed.

It cannot, I think, be doubted that the method of observation here pursued is destined to furnish useful control and guidance in researches of this nature.

In a letter published subsequently in the 'Pall Mall Gazette,' one of the peculiarities of the method pursued in the foregoing lecture is pointed out. Reference is made to the blue colour of the sky, which is, or may be, "produced by particles suspended in the air, and not only invisible to the naked eye, but irreducible by the highest powers of the microscope. Hence, without seeing the individual particles, we may have indubitable evidence of their existence.

"This, indeed, is the point wherein the method pursued in the lecture differs from preceding ones, and is destined powerfully to supplement them.* The microscope seeks for single particles; but they are here taken *en masse*, and their existence demonstrated by the light which they scatter after they have passed utterly beyond the range of the microscope."

Attention was drawn at the same time to the important report of Dr. Angus Smith, published in 1869, which was sent to Professor Tyndall subsequently to his lecture. Pasteur had previously counted the germs of Paris air, but the exceedingly ingenious method employed by Dr. Smith enabled him to concentrate the germs of a very large volume of air in a small quantity of water, and thus to enormously multiply their numbers in relation to the space which contained them:

"I have been favoured," says Mr. Tyndall, "by Dr. Angus Smith with a copy of his fifth annual report, from which I glean some inter-

* It is competent, for example, to give ocular evidence of the absence of germs in still air; and thus to prove the correctness of Pasteur's experiments on the air of the caves under the Paris Observatory.

esting facts regarding the air of Manchester. To catch in water the floating matter of the air, Dr. Smith places a small quantity of the liquid in a bottle and shakes it up with successive charges of air. In one instance he did this 500 times, and then handed over his bottle to an able microscopist, Mr. J. B. Dancer, for examination. The bottle had been shaken in the open air, through which, however, Dr. Smith could not see any dust blowing; at all events, if there were dust, it was only such as people are called upon to breathe. Here are some of the revelations of Mr. Dancer:—

"'*Fungoid Matter.*—Spores or sporidiæ appeared in numbers, and, to ascertain as nearly as possible the numerical proportion of these bodies in a single drop of the liquid, the contents of the bottle were well shaken, and then one drop was taken up with a pipette. This was spread out by compression to a circle half an inch in diameter. A magnifying power was then employed, which gave a field of view of an area exactly $\frac{1}{100}$th of an inch in diameter, and it was found that more than 100 spores were contained in this space. Consequently the average number of spores in a single drop would be 250,000. These spores varied from $\frac{1}{10000}$th to $\frac{1}{50000}$th of an inch in diameter.

"'For the purpose of obtaining a rough approximation to the number of spores or germs of organic matter contained in the entire fluid received from Dr. Smith, I measured a quantity by the pipette, and found it contained 150 drops of the size used in each examination. Now I have previously stated that in each drop there were about 250,000 of these spores, and as there were 150 drops, the sum total reaches the startling number of $37\frac{1}{2}$ millions; and these, exclusive of other substances, were collected from 2495 litres of the air of this city—a quantity which would be respired in about ten hours by a man of ordinary size when actively employed. I may add that there was a marked absence of particles of carbon among the collected matter.'

"Apart from their other effects, the mere mechanical irritation produced by the deposition of these particles in tender lungs must go for something. They may be entirely withheld by a cotton wool respirator. In various dusty trades and occupations the respirator will also be found a comfort and protection."

The employment of cotton wool on scalds and burns; its healing effect on wounds generally; the use of flour in erysipelas; even the binding up of wounds by sticking-plaster, and the covering of them by gold-beaters' skin, may all have their rational ground in the fact that they withhold, not the air, but the organic matter of the air.

The reader will bear in mind that as this subject is not one with which my own studies would have rendered me familiar, I may be uttering that which has been already expressed by others. A similar remark applies to the history of the subject, which, as might be expected, is by no means meagre. Nyander held that small-pox, measles, the plague, dysentery, and hooping-cough, are all caused by minute animals. Réaumur thought that the small clouds which sometimes seem to hug the earth in summer weather may be insect swarms.

Cuvier speaks of the *richesse effrayante* of insect life. Sir H. Holland thinks that the outbreak of carbuncular boils which occurred some years ago in England, may have had its origin without the system, as a virus or some form of organic life. Ehrenberg, whose wonderful investigations have been heard of everywhere, speaks of the "*milky way* of smallest organization." The electric beam renders the figure admirably just. Henle maintained that the material of all contagious diseases is not merely organic, but matter possessing all the characters of parasitic life. Eiselt found pus corpuscles in a foundling hospital where the children were suffering from conjunctival blennorrhœa, and proved conclusively that such corpuscles spread the epidemic without contact with the infected persons. Pouchet, the able and ardent advocate of the doctrine of heterogenesis, has devised an instrument, called an aëroscope, to catch the microscopic particles of the air. This instrument was employed by Eiselt in the foregoing inquiry. Every Alpine man can testify to the correctness of De Saussure's remark that a deep blue heaven portends rain, while the air is rendered turbid by a succession of fine days. De la Rive ascribes this turbidity to organic germs which swathe the earth as a light haze. He has devised a photometer for determining the transparency of such air, and of connecting this with the other elements of meteorology. He also refers to the bearing of the subject on epidemic diseases.

The papers of Dr. Budd, in relation to contagious diseases, are full of interesting facts, and marked by rare logical force. Professor Lister has brought to my notice an observation of his own, the sagacity of which is so strikingly demonstrated by the experiments on the breath recorded in the foregoing lecture, that I propose to give it special attention at a future time. In a pamphlet published in 1850 Mr. Jeffreys reveals some exceedingly unpleasant facts regarding the air of London. He had then ventilated a house with filtered air, and examined the strained matter. I refer to page 16 of his pamphlet for a statement of what that matter is. Dr. Angus Smith's researches on the air of Manchester have been already mentioned. Dr. Smith also experimented on the air of cow-houses and stables, and concluded that such air contains more particles than the air of the street. Mr. Crookes has sought to entrap the germs in infected places. Dr. Greenhow has examined the lungs of stone-workers, colliers, and potters, and found imbedded in them mill-stone dust, silica, alumina, and iron. The important researches of Dr. Stenhouse on the action of charcoal, though not strictly belonging to the present subject, may be mentioned here; and also the experiments of Dr. Marcet.

As may be seen from the foregoing imperfect summary, the history of this subject is voluminous. I shall probably return to it, and give it further expansion.

[J. T.]

WEEKLY EVENING MEETING,

Friday, January 28, 1870.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

WILLIAM ODLING, M.B. F.R.S.
FULLERIAN PROFESSOR OF CHEMISTRY, R.I.

On Professor Graham's Scientific Work.

THE simple story of Mr. Graham's life, though not without its measure of interest, and certainly not without its lessons, is referred to in the following pages only in illustration of the grander story of his work. Thomas Graham was born in Glasgow, on the 21st Dec., 1805. He entered as a student at the University of Glasgow, in 1819, with a view to becoming ultimately a Minister of the Established Church of Scotland. At that time, the University chair of Chemistry was filled by Dr. Thomas Thomson, a man of very considerable mark, and one of the most erudite and thoughtful chemists of his day. The chair of Natural Philosophy was also filled by a man of much learning, Dr. Meikleham, who appears to have taken a warm personal interest in the progress of his since distinguished pupil. Under these masters, Mr. Graham acquired a strong liking for experimental science, and a dislike to the profession chosen for him by his father; who, for a time at least, seems to have exerted the authority of a parent somewhat harshly, but quite unavailingly, to effect the fulfilment of his own earnest wishes in the matter.

After taking his degree of Master of Arts at Glasgow, in 1826, Mr. Graham worked for nearly two years in the laboratory of the University of Edinburgh, under Dr. Hope. He then returned to Glasgow; and, while supporting himself by teaching, at first mathematics and afterwards chemistry, yet found time to follow up the path of experimental inquiry, on which he had already entered.

His first original paper appeared in the 'Annals of Philosophy' for 1826, its author being at that time in his twenty-first year. It is interesting to note that the subject of this communication, "On the Absorption of Gases by Liquids," forms part and parcel of that large subject of spontaneous gas-movement with which Mr. Graham's name is now so inseparably associated; and that, in a paper communicated to the Royal Society just forty years later, he speaks of the lique-

fiability of gases by chemical means, in language almost identical with that used in this earliest of his published memoirs.

Having, in the interval, contributed several other papers to the scientific journals, in the year 1829 he published in the 'Quarterly Journal of Science'—the journal, that is to say, of the Royal Institution—the first of his papers relating specifically to the subject of gas-diffusion. It was entitled "A short Account of Experimental Researches on the Diffusion of Gases through each other, and their Separation by Mechanical Means." In the same year, he became Lecturer on Chemistry at the Mechanics' Institute, Glasgow; and in the next year, 1830, achieved the yet more decisive step of being appointed Professor of Chemistry at the Andersonian University. By this appointment he was relieved from anxiety on the score of living; and afforded, in a modest way, the means of carrying out his experimental work.

In 1831, he read, before the Royal Society of Edinburgh, a paper "On the Law of the Diffusion of Gases," for which the Keith prize of the Society was shortly afterwards awarded him. Although several of his earlier papers, and especially that on the Diffusion of Gases, published in the 'Quarterly Journal of Science,' had given evidence of considerable power, it was this paper—in which he established the now well-recognized law that the velocities of diffusion of different gases are inversely as the square roots of their specific gravities—that constituted the first of what may properly be considered his great contributions to the progress of chemical science.

In 1833, he communicated a paper, of scarcely less importance, to the Royal Society of London, entitled "Researches on the Arseniates, Phosphates, and Modifications of Phosphoric Acid." It afforded further evidence of Mr. Graham's quiet steady power of investigating phenomena, and of his skill in interpreting results; or rather of his skill in setting forth the results in all their simplicity, undistorted by the gloss of preconceived notions, so as to make them render up their own interpretation. It is difficult now-a-days to realize the independence of mind involved in Mr. Graham's simple interpretation of the facts, presented to him in this research, by the light of the facts themselves, irrespective of all traditional modes of viewing them. Their investigation let in a flood of light upon the chemistry of that day; and formed a starting-point from which many of our most recent advances may be directly traced. In this paper, Mr. Graham established the existence of two new, and, at that time, wholly unanticipated classes of bodies, namely, the class of polybasic acids and salts, and the class of so-called anhydro-acids and salts. The views of Graham on the polybasicity of phosphoric acid were soon afterwards applied by Liebig to tartaric acid, and by Gerhardt to polybasic acids in general, as we now recognize them. After a long interval, the idea of polybasicity was next extended to radicals and to metals by Williamson and myself successively; afterwards to alcohols by Wurtz, and to ammonias by Hofmann. The

notion of anhydro-salts was extended by myself to the different classes of silicates; by Wurtz to the compounds intermediate between oxide of ethylene and glycol; and by other chemists to many different series of organic bodies.

The next most important of the researches, completed by Mr. Graham while at Glasgow, was the subject of a paper communicated to the Royal Society of Edinburgh, in 1835, "On Water as a Constituent of Salts," and of a second paper communicated to the Royal Society of London, in 1836, entitled "Inquiries respecting the Constitution of Salts, &c.," for which latter, a Royal medal of the Society was afterwards awarded. The subject of hydration had yielded him such a harvest of results in the case of phosphoric acid, that it was only natural he should wish to pursue the inquiry farther. Indeed, it is a curious illustration of the persistency of the man, that he never seems to have left out of sight the subjects of his early labours. Almost all his subsequent original work is but a development, in different directions, of his youthful researches on gas-diffusion and water of hydration; and so completely did he bridge over the space intervening between these widely remote subjects, that, with regard to several of his later investigations, it is difficult to say whether they are most directly traceable to his primitive work on the one subject or on the other.

In 1837, on the death of Dr. Edward Turner, Mr. Graham was appointed Professor of Chemistry at University College, London, then called the University of London. On his acceptance of this appointment, he began the publication of his well-known 'Elements of Chemistry,' which appeared in parts, at irregular intervals, between 1837 and 1841. Elementary works, written for the use of students, have necessarily much in common; but the treatise of Mr. Graham, while giving an admirably digested account of the most important individual substances, was specially distinguished by the character of the introductory chapters, devoted to Chemical Physics; wherein was set forth one of the most original and masterly statements of the first principles of chemistry that has ever been placed before the English student. "The Theory of the Voltaic Circle" had formed the subject of a paper communicated by Mr. Graham to the British Association in 1839; and the account of the working of the battery, given in his Elements of Chemistry, and based on the above paper, will long be regarded as a model of lucid scientific exposition.

In 1841, the now flourishing Chemical Society of London was founded; and though Mr. Graham had been, at that time, but four years in London, such was the estimation in which he was held by his brother chemists, that he was unanimously chosen as the first President of the Society. The year 1844 is noticeable in another way. Wollaston and Davy had been dead for some years. Faraday's attention had been diverted from chemistry to those other branches of experimental inquiry in which his highest distinctions were achieved; and, by the death of Dalton in this year, Mr. Graham was left as the acknowledged

first of English chemists, as the not unworthy successor to the position of Black, Priestley, Cavendish, Wollaston, Davy, and Dalton.

From the period of his appointment at University College, in 1837, Mr. Graham's time was fully occupied in teaching, in writing, in advising on chemical manufactures, in investigating fiscal and other questions for the Government, and in the publication of various scientific memoirs, several of them possessing a high degree of interest; but it was not till 1846 that he produced a research of any considerable magnitude. In that year he presented to the Royal Society the first part of a paper "On the Motion of Gases," the second part of which he supplied in 1849. For this research, Mr. Graham was awarded a second Royal Medal of the Society in 1850. The preliminary portion of the first part of the paper related to an experimental demonstration of the law of the effusion of gases, deduced from Torricelli's theorem on the efflux of liquids,—a demonstration that was achieved by Mr. Graham with much ingenuity, and without his encountering any formidable difficulty. But the greater portion of the first part, and whole of the second part, of this most laborious paper, were devoted to an investigation of the velocities of transpiration of different gases through capillary tubes; with a view to discover some general law by which their observed transpiration rates might be associated with one another. Again and again, with characteristic pertinacity, Mr. Graham returned to the investigation; but, although much valuable information of an entirely novel character was acquired —information having an important bearing on his subsequent work— the problem itself remained, and yet remains, unsolved. Why, for example, under an equal pressure, oxygen gas should pass through a capillary tube at a slower rate than any other gas is a matter that still awaits interpretation.

Near the end of the same year, 1849, Mr. Graham communicated, also to the Royal Society, a second less laborious, but in the novelty and interest of its results more successful, paper "On the Diffusion of Liquids." It was made the Bakerian lecture for 1850; and was supplemented by further observations communicated to the Society in 1850 and 1851. In his investigation of this subject, Mr. Graham applied to liquids the exact method of inquiry which he had applied to gases just twenty years before, in that earliest of his papers on the subject of gas-diffusion published in the 'Quarterly Journal of Science;' and he succeeded in placing the subject of liquid-diffusion on about the same footing as that to which he had raised the subject of gas-diffusion prior to the discovery of his numerical law.

In 1854, Mr. Graham communicated another paper to the Royal Society, "On Osmotic Force," a subject intimately connected with that of his last previous communication. This paper was also made the Bakerian lecture for the year; but, altogether, the conclusions arrived at were hardly in proportion to the very great labour expended on the inquiry. In the next year, 1855, just five-and-twenty years after his appointment at the Andersonian University, Mr. Graham was

made Master of the Mint; and, as a consequence, resigned his Professorship at University College. During the next five years he published no original work.

Thus, at the beginning of the year 1861, Mr. Graham, then fifty-six years of age, had produced, in addition to many less important communications, five principal memoirs; three of them in the highest degree successful; the other two less successful in proportion to the expenditure of time and labour on them, but, nevertheless, of great originality and value. The most brilliant period, however, of his scientific career was to come. In the year 1861, and between then and his death in 1869, Mr. Graham communicated four elaborate papers to the Royal Society; three of them far exceeding in novelty, interest, and philosophic power anything that he had before produced; and the other of them, relating to a certain physical effect of that hydration of compounds, from the consideration of which his attention could never wholly be withdrawn. This least important paper, "On Liquid Transpiration in relation to Chemical Composition," was communicated to the Royal Society in 1861. Of the three greater papers, that "On Liquid Diffusion applied to Analysis" was communicated also in 1861. For this paper more especially, as well as for his Bakerian lectures "On the Diffusion of Liquids" and "On Osmotic Force," Mr. Graham received, in 1862, the Copley Medal of the Royal Society; and, in the same year, was also awarded the Jecker Prize of the Institute of France. Following in quick succession, his paper "On the Molecular Mobility of Gases" was presented to the Royal Society in 1863; and that "On the Absorption and Dialytic Separation of Gases by Colloid Septa," in 1866. With regard to these three great papers, two of them were each supplemented by a communication to the Chemical Society; while the third was supplemented by four successive notes to the Royal Society, containing an account of further discoveries on the same subject, hardly less remarkable than those recorded in the original paper. The last of these supplementary notes was communicated on June 10th, 1869, but a few months before the death, on September 13th, of the indefatigable but physically broken-down man.

In considering Mr. Graham as a chemical philosopher and lawgiver we find him characterized by a pertinacity of purpose peculiarly his own. Wanting the more striking qualities by which his immediate predecessors, Davy, Dalton, and Faraday, were severally distinguished, he displayed a positive zeal for tedious quantitative work, and a wonderful keen-sightedness in seizing the points which his innumerable determinations of various kinds, conducted almost incessantly for a period of forty years, successively unfolded. His work itself was essentially that of detail, original in conception, simple in execution, laborious by its quantity, and brilliant in the marvellous results to which it led. As regards its simplicity of execution, scarcely any investigator of recent times has been less a friend to the instrument-maker than Mr. Graham. While availing himself,

with much advantage, of appliances devised by Bunsen, Poiseuille, Sprengel, and others, all the apparatus introduced by himself was of the simplest character, and for the most part of laboratory construction.

Essentially inductive in his mode of thought, Mr. Graham developed his leading ideas, one after another, directly from experiment, scarcely, if at all, from the prevailing ideas of the time. As well observed by Dr. Angus Smith, "he seemed to feel his way by his work." His records of work are usually, in a manner almost characteristic, preceded each by a statement of the interpretation or conclusion which he formed; but the records themselves are expressed in the most unbiassed matter-of-fact language. Singularly cautious in drawing his conclusions, he announces them from the first with boldness, making no attempt to convince, but leaving the reader to adopt them or not as he pleases. Accordingly, in giving an account of his various researches, Mr. Graham rarely, if ever, deals with argument; but he states succinctly the experiments he has made, the conclusions he has himself drawn, and not unfrequently the almost daring speculations and generalizations on which he has ventured. Some of these speculations, on the constitution of matter, are reproduced in his own words farther on.

Mr. Graham was elected a Fellow of the Royal Society in 1837; Corresponding Member of the Institute of France in 1847; and Doctor of Civil Law of Oxford in 1855.

The remaining pages of this abstract are devoted to an account of his principal discoveries,—the generalizations they suggested to him, and the relations in which they stood to precedent knowledge.

I.

Modifications of Phosphoric Acid.—At the date of Mr. Graham's investigation of this subject, when oxy-salts were usually represented as compounds of anhydrous base with anhydrous acid, the point of greatest importance, with regard to each class of salts, was held to be the ratio borne by the oxygen of the base to the oxygen of the acid. Thus, in the carbonates, this ratio was as 1 to 2; in the sulphates, as 1 to 3; and in the nitrates, as 1 to 5. But with regard to the phosphates, taking common phosphate of soda as a type of phosphates in general, there was a difficulty. Dr. Thomson maintained that, in this salt, the ratio of the oxygen of the base to the oxygen of the acid was as 1 to 2; and his view was substantially supported by Sir Humphry Davy. Berzelius contended, however, that the ratio was as 1 to $2\frac{1}{2}$, or, to avoid the use of fractions, as 2 to 5; but, notwithstanding the excellence of the Swedish chemist's proof, and its corroboration by the researches of others, the simpler and, as it seemed, more harmonious view of Dr. Thomson prevailed very generally in this country. Anyhow, those phosphates in which the oxygen ratio was the same as that in phosphate of soda were taken as the neutral

salts. But phosphate of soda was found to have the peculiar and quite inexplicable property of reacting with nitrate of silver to throw down, as a yellow precipitate, a phosphate of silver, in which the proportion of metallic base exceeded that in the original phosphate of soda,—the precipitation of the basic salt being accompanied correlatively by the formation of a strongly acid liquid. According to Berzelius, the ratio of the oxygen of the base to that of the acid, in this yellow precipitate, was as 3 to 5.

In 1821, Mitscherlich, then working in Berzelius's laboratory, obtained, by treating ordinary phosphate of soda with aqueous phosphoric acid, a new crystallizable phosphate of soda, in which the ratio of acid to base was twice as great as that in the ordinary phosphate. This new salt, which had a strongly acid reaction to test paper, he called the bi-phosphate of soda. He observed that it was a hydrated salt; and that while the ratio, in it, of the oxygen of the base to the oxygen of the acid was as 1 to 5, the ratio of the oxygen of the base to the oxygen of the water was as 1 to 2.

In 1827, Mr. Graham's fellow-townsman, and predecessor at the Mechanics' Institute, Dr. Clark, discovered another new phosphate of soda, in which the ratio of the oxygen of the base to the oxygen of the acid was identical with that in the ordinary phosphate, namely, as 2 to 5. But whereas the ordinary phosphate crystallized with 25 proportions of water, the new phosphate crystallized with only 10; and whereas the ordinary phosphate gave a yellow precipitate with nitrate of silver and a strongly acid supernatant liquid, the new phosphate gave a chalk-white precipitate with nitrate of silver and a perfectly neutral supernatant liquid. This new phosphate, being formed by heating the common phosphate to redness, was accordingly designated the pyrophosphate. By dissolution in water and evaporation of the liquid, it could be obtained in the 10-hydrated crystalline state; and by desiccation at a sand-bath heat, the crystalline salt could be again rendered anhydrous. With regard to the 25 proportions of water belonging to the ordinary salt, Dr. Clark noticed that 24 proportions could be driven off by a sand-bath heat, and that this moderate heat did not alter the nature of the salt. He found that the 25th proportion of water, however, could only be driven off by a full red heat; and that, simultaneously with its expulsion, the change in the nature of the salt was effected. But he carefully guarded himself against being supposed to think that the change in properties of the salt was consequent upon an elimination of its water. The driving off of water from salts being, as he justly remarked, a common effect of heat, he regarded this effect as a concomitant only of the peculiar effect of heat in altering the nature of the phosphate.

Other anomalies with regard to phosphoric acid and the phosphates were also known to chemists; and, on referring now to standard chemical works written before the year 1833, the whole subject of the phosphates will be seen to be in the greatest confusion. It was in this year that Mr. Graham communicated his paper, entitled "Researches

on the Arseniates, Phosphates, and Modifications of Phosphoric Acid," to the Royal Society.*

In the course of these researches he established the existence of a class of soluble sub-phosphates analogous to the yellow insoluble phosphate of silver; and he showed, with great clearness, that in the three classes of phosphates, namely, the sub-phosphates, the common phosphates, and the bi-phosphates, the ratio borne to the oxygen of the acid by the other oxygen of the salt is the same, namely, as 3 to 5; only that, in the three classes of salts, the non-acid oxygen is divided between different proportions of metallic base and water, thus:

Sub-phosphate of soda	$3NaO.PO_5$.
Common phosphate of soda	$HO.2NaO.PO_5$.
Bi-phosphate of soda	$2HO.NaO.PO_5$.

He further pointed out that, to these three series of salts, there corresponded a definite phosphate of water, or,

Hydrated phosphoric acid	$3HO.PO_5$.

Compounds of one and the same anhydrous acid with one and the same anhydrous base, in different proportions, had long been known; but it was thus that Mr. Graham first established the notion of poly-basic compounds,—the notion of a class of hydrated acids having more than one proportion of water replaceable by metallic oxide, and that successively, so as to furnish more and more basic salts, all preserving, as we should now say, the same type.

Mr. Graham further showed that Dr. Clark's pyrophosphate of soda, like the common phosphate, yielded an acid-salt or bi-phosphate; and that these two compounds were related to a hydrated phosphoric acid, differing in composition and properties from the above-mentioned hydrate, and yielding, after neutralization with alkali, a white instead of a yellow precipitate with nitrate of silver. This series of compounds he expressed by the following formulæ:

Clark's pyrophosphate of soda	$2NaO.PO_5$.
Acid or bi-pyrophosphate of soda	$HO.NaO.PO_5$.
Hydrated pyrophosphoric acid	$2HO.PO_5$.

Lastly, Mr. Graham showed that when the bi-phosphate or bi-pyrophosphate of soda was ignited, there was left a new variety of phosphate, which he called the metaphosphate, having the same proportions of soda and anhydrous phosphoric acid as the original compound, but differing from it in several properties, more particularly in its inability to furnish any acid salt. From this new phosphate he obtained the corresponding hydrated acid, and found it to be identical with that variety of phosphoric acid then, and still, known as glacial phosphoric acid; which had previously been noticed to possess

* 'Phil. Trans.,' 1833, p. 253.

the distinctive property of causing a precipitate in solutions of albumen. This salt and acid he represented as follows:—

Metaphosphate of soda	$NaO.PO_5$.
Metaphosphoric acid	$HO.PO_5$.

Speaking of the acid obtainable from, and by its neutralization reconverted into, the phosphate, pyrophosphate, and metaphosphate of soda respectively, Mr. Graham remarked:—"The acid, when separated from the base, will possess and retain for some time the characters of its peculiar modification. . . . . But I suspect that the modifications of phosphoric acid, when in what we would call a free state, are still in combination with their usual proportion of base, and that that base is water. Thus the three modifications of phosphoric evidence may be composed as follows:—

Phosphoric acid	$3HO.PO_5$
Pyrophosphoric acid	$2HO.PO_5$
Metaphosphoric acid	$HO.PO_5$;

or they are respectively a triphosphate, a biphosphate, and phosphate of water." These remarks he followed up by analytical evidence, showing the existence of the three hydrates, each in its isolated state.

Just as in his demonstration of the relationship to one another of sub-phosphate of soda, phosphate of soda, bi-phosphate of soda, and common phosphoric acid, Mr. Graham originated the notion of polybasic compounds, so, in his demonstration of the nature of the pyrophosphates and metaphosphates, as bodies differing from the normal compounds by an abstraction of water or metallic base, did he originate the notion of anhydro-compounds—so did he discover, for the first time, an instance of that relationship between bodies which is now known to prevail most extensively among products of organic as well as of mineral origin.

The different properties manifested by phosphoric acid, in its different reputedly isomeric states, having been shown by Mr. Graham to be dependent on a difference of hydration, that is to say on a difference of chemical composition, he was inclined to view the difference of properties observed in the case of other reputedly isomeric bodies as being also dependent on a difference of composition, the difference occasionally consisting in the presence of some minute disregarded impurity. Accordingly, he communicated to the Royal Society of Edinburgh in 1834,* a paper "On Phosphuretted Hydrogen," in which he showed that the spontaneously inflammable and non-spontaneously inflammable varieties of the gas "are not isomeric bodies, but that the peculiarities of the spontaneously inflammable species depend upon the presence of adventitious matter," removable in various ways,

* 'Edin. Roy. Soc. Trans.,' xiii., 1836, p. 88.

and existing but in very minute proportion.* He further showed that the vapour of some acid of nitrogen, apparently "nitrous acid, is capable of rendering phosphuretted hydrogen spontaneously inflammable when present to the extent of *one ten-thousandth part* of the volume of the gas." In connection with this research, may be mentioned Mr. Graham's earlier experiments on the influence of minute impurities in modifying the chemical behaviour of different substances. In some "Observations on the Oxidation of Phosphorus," published in the 'Quarterly Journal of Science,'† for 1829, he showed that the presence of $\frac{1}{150}$ of olefiant gas, and even $\frac{1}{4400}$, by volume, of turpentine vapour, in air under ordinary pressure, rendered it incapable of effecting the slow oxidation of phosphorus. He also observed and recorded the influence upon the oxidation of phosphorus of various additions of gas and vapour to air, under different circumstances of pressure and temperature.

II.

Hydration of Compounds.—In the earliest of Mr. Graham's published memoirs, that "On the Absorption of Gases by Liquids,"‡ he contended that the dissolution of gases in water, at any rate of the more soluble gases, is a chemical phenomenon, depending on their essential property of liquefiability being brought into play by their reaction with the solvent, that is to say by their hydration. The results of some further work on the same subject he published under the title of "Experiments on the Absorption of Vapours by Liquids."§

In 1827, he gave to the Royal Society of Edinburgh "An Account of the Formation of Alcoholates, definite Compounds of Salts and Alcohol analogous to the Hydrates."‖ In this paper, after a description of some experiments on the desiccation of alcohol, he showed that anhydrous chloride of calcium, nitrate of lime, nitrate of magnesia, chloride of zinc, and chloride of manganese have the property of uniting with alcohol, as with water, to form definite compounds. The crystalline compound with chloride of zinc, for instance, containing 15 per cent. of alcohol, he represented by the formula $ZnCl.\ 2C_2H_6O$; corresponding to the modern formula $ZnCl_2.\ 2C_2H_6O$.

In 1835, Mr. Graham communicated a paper, also to the Royal Society of Edinburgh, "On Water as a Constituent of Salts."¶ In this paper he showed more particularly that the so-called magnesian sulphates, crystallizing usually with 7, 6, or 5 proportions of water, gave up all but the last proportion of water at a moderate heat, but retained this last proportion with great tenacity. The comparatively stable mono-hydrated salts, — monohydrated sulphate of zinc, for

* It was afterwards isolated by P. Thenard.
† 'Quart. Journ. Sci.,' ii., 1829, p. 83.
‡ Thomson, 'Ann. Phil.,' xii., 1826, p. 69.
§ 'Edin. Journ. Sci.,' viii., 1828, p. 326.
‖ 'Edin. Roy. Soc. Trans.,' xi., 1837, p. 175.
¶ Ibid., xiii., 1836, p. 297.

instance, $ZnO.SO_3.HO$,—he regarded as the analogues of crystallizable sulphuric acid $HO.SO_3.HO$. He showed further that the firmly-retained water of sulphate of zinc, for instance, differed from the firmly-retained water of phosphate of soda, in not being basic, or replaceable, that is to say, by metallic oxide. He conceived, however, that in the double sulphates, potassio-sulphate of zinc, for instance, $ZnO.SO_3$, $KO.SO_3$, the water of the compound, $ZnO.SO_3.HO$, was replaced by alkali-sulphate, and he accordingly designated the water of this last, and of similar compounds, by the name of saline or constitutional water.

In the following year, 1836, Mr. Graham communicated to the Royal Society of London an elaborate paper, entitled "Inquiries respecting the Constitution of Salts, of Oxalates, Nitrates, Phosphates, Sulphates, and Chlorides."* In it are recorded careful analyses of very many salts, more particularly in respect to their water of hydration; with remarks upon the greater or less tenacity with which the water is retained in different instances. In this paper he put forward the notion that truly basic salts are nevertheless neutral in constitution; and that the excess of metallic base does not stand in the relation of a base to the anhydrous acid, but as a representative of the water of hydration of the neutral salt. He illustrated this position by a comparison of the definite hydrate of nitric acid with other hydrated nitrates, thus:

Hydrated nitric acid, sp. gr. 1·42	$HO.NO_5.3\,HO$
Hydrated nitrate of zinc	$ZnO.NO_5.3\,HO$
Hydrated nitrate of copper	$CuO.NO_5.3\,HO$
Basic nitrate of copper	$HO.NO_5.3\,CuO$

He contended that, in the last cupric salt, it is the water and not the oxide of copper which acts as a base; and, in support of this view, he remarked that if the water of the salt were water of hydration simply, it ought, in presence of so large an excess of metallic base, to be very readily expelled by heat; whereas it is actually inexpulsable by any heat whatever, short of that effecting an entire decomposition of the salt. Again, he pointed out that when the strongest nitric acid $HO.NO_5$, is added, in no matter what excess, to oxide of copper, the basic salt is alone produced, apparently by a direct addition of the oxide of copper to the nitrate of water.

In 1841, Mr. Graham gave to the Chemical Society "An Account of Experiments on the Heat disengaged in Combination."† These experiments included numerous determinations of the heat evolved in the hydration of salts, and more particularly of the sulphates, including sulphate of water, or hydrated sulphuric acid. Starting from oil of vitriol $HO.SO_3$, he found that each successive addition of a proportion of water HO, evolved an additional, but successively

* 'Phil. Trans.,' 1837, p. 47. † 'Chem. Soc. Mem.,' i., p. 106.

smaller and smaller increment of heat; and that, even after the addition of fifty proportions of water to the acid, the further addition of water was yet followed by a perceptible development of heat.

The relation of ether to alcohol being regarded as that of an oxide to its hydrate, and expressed by the formulæ C_4H_5O, and $C_4H_5O.HO$, the conversion of alcohol into ether became a matter of dehydration; and, accordingly, could not escape the examination of Mr. Graham, who, in 1850, presented to the Chemical Society some "Observations on Etherification."* The process of manufacture consisting in the distillation of a mixture of alcohol with sulphuric acid, and being attended by an intermediate production of sulphate of ether or sulphethylic acid, the substitution of ether for the basic water of sulphuric acid at one temperature, and the reverse substitution of water for the basic ether of sulphethylic acid at a higher temperature, had been represented as depending on the augmented elasticity of the ether vapour at the higher temperature. Mr. Graham showed, however, that ether could be very readily formed by heating the mixture of sulphuric acid and alcohol in sealed tubes—that is, under conditions in which the augmentation of volatility due to heat was *pari passu* counterbalanced by the diminution of volatility due to pressure. Altogether, Mr. Graham supported the contact theory of ether formation, as opposed to the then received reaction theory; but several of his experiments afforded clear, though indeed supererogatory, support to the reaction theory soon afterwards introduced by Williamson.

In addition to the memoirs cited above, the question of hydration formed an express or incidental subject of many other of Mr. Graham's investigations. It is noteworthy that, for him, osmosis became a mechanical effect of the hydration of the septum; that the interest attaching to liquid-transpiration was the alteration in rate of passage consequent on an altered hydration of the liquid; that the dialytic difference between crystalloids and colloids depended on the dehydration of the dialytic membrane by the former class of bodies only; and similarly in many other instances.

III.

Movements of Liquids under Pressure. Transpiration.—That the velocities with which different liquids, under the same pressure, issue from a hole in the side or bottom of a vessel should be inversely as the square roots of their respective specific gravities is a proposition deducible from well-known mechanical principles. As demonstrated, however, by Dr. Poiseuille, this law is not applicable to the case of liquids issuing under pressure through capillary tubes. In addition to determining experimentally the laws of the passage of the same liquid—that the velocity is directly as the pressure, inversely as the length of the capillary, and directly as the fourth power of the diameter; and that it is accelerated by elevation of temperature—

* 'Chem. Soc. Journ.,' iii., p. 24.

Dr. Poiseuille further showed that the rate of passage of different liquids through capillary tubes is for the most part a special property of the particular liquids; and that while the rate of passage of water, for instance, is scarcely affected by the presence of certain salts in solution, it is materially accelerated by the presence of chlorides and nitrates of potassium and ammonium, and materially retarded by the presence of alkalies. He also showed that while the rate of passage of absolute alcohol is much below that of water, the rate of passage of alcohol diluted with water in such proportion as to form the hydrate, $H_6C_2O.3Aq$, is not only much below that of alcohol, but also below that of any other mixture of alcohol and water.

Some time after Dr. Poiseuille's death, Mr. Graham, starting from this last observation, took up the inquiry. Giving to the phenomenon itself the name of "transpiration," which he had previously applied to the similar passage of gases through capillary tubes, he communicated his results to the Royal Society in a paper "On Liquid Transpiration in relation to Chemical Composition."* The method he followed in his experiments was precisely that of Dr. Poiseuille, and the principal results at which he arrived are the following:—

1. That dilution with water does not effect a *pari passu* alteration in the transpiration velocity of certain liquids; but that dilution up to a certain point, corresponding to the formation of a definite hydrate, not unfrequently retards the transpiration velocity (or increases the transpiration time) to a maximum, from which the retardation gradually diminishes with further dilution. This is well seen in the following Table, giving the transpiration times of certain liquids in their undiluted state, and also the maximum transpiration times observed with the same liquids when diluted with a regularly increasing quantity of water, the particular dilution causing the maximum retardation corresponding in every case to the production of a definite hydrate.

		Transpiration Times.		
Water	H_2O ..	1·000	1·000	x Aq.
Sulphuric acid ..	H_2SO_4 ..	21·651	23·771	H_2SO_4. Aq.
Nitric acid	HNO_3 ..	·990	2·103	$2\,HNO_3$.3 Aq.
Acetic acid	$H_4C_2O_2$ ..	1·280	2·704	$H_4C_2O_2$.2 Aq.
Alcohol	H_6C_2O ..	1·195	2·787	H_6C_2O.3 Aq.
Wood-spirit	$H_4C\,O$ ..	·630	1·802	$H_4C\,O$.3 Aq.
Acetone	H_6C_3O ..	·401	1·604	H_6C_3O.6 Aq.

2. That the transpiration times of homologous liquids increase regularly with the complexity of the several molecules constituting terms of the same series—certain first terms of the different series,

* 'Phil. Trans.,' 1861, p. 373.

however, presenting some anomalies, as was indeed to be expected. The transpiration times of the fatty ethers are given below in illustration. Similar results were obtained with the series of fatty acids and their corresponding alcohols.

			Transpiration Times.
	Water	H_2O ..	1·000
Ethers	Formic	$H_6C_3O_2$ ..	·511
	Acetic	$H_8C_4O_2$ ..	·553
	Butyric	$H_{12}C_6O_2$ ..	·750
	Valeric	$H_{14}C_7O_2$ ..	·827

In this paper, Mr. Graham also recorded the results of two very full series of determinations of the transpiration rates of water at different temperatures between 0° and 70°; and of two similar series of experiments made with alcohol. The transpiration velocity of water was found to increase uniformly from 0·559 at 0° to 1·000 at 20°, and thence to 2·350 at 70°; and correlatively the transpiration times were found to decrease in the same proportion. The results obtained with alcohol were precisely similar.

IV.

Diffusion of Liquids.—Mr. Graham's early study of the spontaneous movements of gases, so as to mix with one another, naturally led him to investigate the similarly occurring movements of liquids. His results formed the subject of two papers communicated to the Royal Society, one in 1849 "On the Diffusion of Liquids,"* and the other in 1861 "On Liquid Diffusion applied to Analysis."† In the series of experiments, described in the first of these papers and in two supplementary communications, an open wide-mouthed vial, filled with a solution of some salt or other substance, was placed in a jar of water; when, in course of time, a portion of the dissolved salt, described as the diffusate, passed gradually from the vial into the external water. By experimenting in this manner, the amounts of diffusate yielded by different substances were found to vary greatly. Thus, under precisely the same conditions, common salt yielded twice as large a diffusate as Epsom salt, and this latter twice as large a diffusate as gum-arabic. Every substance examined was in this way found to have its own rate of diffusibility in the same liquid medium—the rate varying with the nature of the medium—whether water or alcohol, for instance. It is noticeable that the method of vial-diffusion resorted to in these experiments, is exactly similar to that employed by Mr. Graham in

* 'Phil. Trans.,' 1850, pp. 1, 805; 1851, p. 483.
† Ibid., 1861, p. 183.

his earliest experiments on the diffusion of gases, published in the 'Quarterly Journal of Science' for 1829.

In the series of experiments recorded in the paper "On Liquid Diffusion applied to Analysis," the solution of the salt to be diffused, instead of being placed in a vial, was conveyed by means of a pipette to the bottom of a jar of water; when, in course of time, the dissolved salt gradually rose from the bottom, through the superincumbent water, to a height or extent proportional to its diffusibility. The results of this method of jar-diffusion were found to bear out generally those attained by the method of vial-diffusion; while they further showed the absolute rate or velocity of the diffusive movement. Thus, during a fourteen days' aqueous diffusion from ten per cent. solutions of gum-arabic, Epsom salt, and common salt respectively, the gum-arabic rose only through $\frac{7}{14}$ths of the superincumbent water, or to a height of 55·5 millimètres; the Epsom salt rose through the whole $\frac{14}{14}$ths of superincumbent water, or to a height of 111 millimètres; and the common salt not only rose to the top, but would have risen much higher, seeing that the uppermost or fourteenth stratum of water, into which it had diffused, contained about fifteen times as much salt as was contained in the uppermost or fourteenth stratum of water into which the Epsom salt had diffused.

But of all the results obtained, the most interesting, from their bearing on various natural phenomena, were those on the partial separation of different compounds from one another, brought about by their unequal diffusibility. Thus, with a solution of equal weights of common salt and gum-arabic placed in the diffusion-vial, for every 100 milligrammes of salt not more than 22·5 milligrammes of gum were found to pass into the external water; or a separation of the salt from the gum, to this large extent, took place spontaneously by the excess of its own proper diffusive movement. Again, when a solution, containing five per cent. of common salt and five per cent. of Glauber's salt, was submitted for seven days to the process of jar-diffusion, the upper half, or $\frac{7}{14}$ths, of superincumbent water was found to contain 380 milligrammes of common salt and only 53 grammes of Glauber's salt; or the ratio of common salt to Glauber's salt in the upper half of the liquid was as 100 to 14, the ratio in the original stratum of solution being as 100 to 100. And not only a partial separation of mixed salts, but even a partial decomposition of chemical compounds was found to result from the process of liquid diffusion. Thus the double sulphate of potassium and hydrogen, when submitted to diffusion, underwent partial decomposition into the more diffusible sulphate of hydrogen and the less diffusible sulphate of potassium; and, similarly, ordinary alum, a double sulphate of aluminum and potassium, underwent partial decomposition into the more diffusible sulphate of potassium, and the less diffusible sulphate of aluminum. Strictly speaking, perhaps, the decomposition of the original salts was not caused by, but only made evident by, the difference in diffusibility of the products.

As a general result of his experiments, Mr. Graham inferred that liquid diffusibility is not associated in any definite way with chemical composition or molecular weight. Thus he found the complex organic bodies picric acid and sugar to have much the same diffusive rates as common salt and Epsom salt respectively. Isomorphous compounds, however, proved for the most part to be equi-diffusive; although the groups of equi-diffusive substances habitually comprehended other than those which were isomorphous.

Observing further that, in many cases, the diffusion-rates of different equi-diffusive groups stood to one another in some simple numerical relation, Mr. Graham remarked that, "In liquid diffusion we no longer deal with chemical equivalents or the Daltonian atoms; but with masses even more simply related to each other by weight." We may suppose that the chemical atoms "group together in such numbers as to form new and larger molecules of equal weights for different substances, or . . . . . of weights which appear to have a simple relation to each other;" and he inferred that the relative weights of these new molecules would be inversely as the square roots of the observed diffusion rates of the substances—that is inversely as the squares of their diffusion times. Thus the squares of the times of equal diffusion of hydrate, nitrate, and sulphate of potassium being 3, 6, and 12, the densities of their diffusion molecules would be as the reciprocals of these numbers, or as 4, 2, and 1.

Lastly, in comparing highly diffusive substances on the one hand, with feebly diffusive substances on the other, one broad dissimilarity became apparent, namely, that highly diffusible substances affected the crystalline state, while feebly diffusive substances were amorphous, and characterized, in particular, by a capability of forming gelatinous hydrates. Hence the distinction established by Mr. Graham between highly diffusive bodies, or *crystalloids*, and feebly diffusive bodies, or *colloids*. Compounds capable of existing both in the crystalline and gelatinous states he found to be possessed of two distinct diffusive rates corresponding respectively each to each.

V.

Dialysis and Osmose.—The subject of dialysis was included in the paper "On Liquid Diffusion applied to Analysis," referred to in the preceding section; and some further results were communicated, in 1864, to the Chemical Society, in a paper "On the Properties of Silicic Acid, and other analogous Colloidal Substances." *

In the course of his experiments on diffusion, Mr. Graham made the curious discovery that highly diffusible crystalloid bodies were able to diffuse readily, not only into free water, but also into water that was already in a low form of combination, as in the substance of a soft solid, such as jelly or membrane. Common salt, for instance, was

* 'Chem. Soc. Journ.,' xvii., p. 318.

found to diffuse into a semi-solid mass of jelly, almost as easily and as extensively as into a similar bulk of free water; but the introduction of a gelatinous substance, though not interfering in any appreciable degree with the diffusion of a crystalloid, was found to arrest almost entirely the diffusion of a colloid. The colloid, of but little tendency to diffuse into free water, proved quite incapable of diffusing into water that was already in a state of combination, however feeble. Hence, although the partial separation of a highly diffusible from a feebly diffusible substance might be effected by the process of free diffusion into water, a much better result was obtained by allowing the diffusion to take place into, or through, the combined water of a soft solid such as a piece of membrane or parchment-paper. In the process of dialysis, then, crystalloid and colloid bodies, existing in solution together, are separated from one another by pouring the mixed solution into a shallow tray of membrane or parchment-paper, and letting the tray rest on the surface of a considerable excess of water, once or twice renewed. By this means the crystalloid, in process of time, diffuses completely away through the membranous septum into the free water; but the colloid, being quite incapable of permeating the membrane, however thin, is retained completely on the tray, unable to reach the free water on the other side.

By means of the process of dialysis, Mr. Graham succeeded in obtaining various colloid organic substances, such as tannin, albumen, gum, caramel, &c., in a very pure state; some of them, indeed, in a state of purity exceeding any in which they had before been met with. But the most curious results were obtained with different mineral substances, usually thrown down from their dissolved salts in the state of gelatinous or colloid precipitates. Most of these precipitates being soluble in some or other crystalloid liquid, on submitting the so-produced solutions to dialysis, the crystalloid constituents diffuse away, leaving the colloid substances in pure aqueous solution. By proceeding in this manner, Mr. Graham was able to obtain certain hydrated forms of silica, ferric oxide, alumina, chrome, prussian blue, stannic acid, titanic acid, tungstic acid, molybdic acid, &c., &c., in the state of aqueous solution,—these bodies having never before been obtained in solution, save in presence of strongly acid or alkaline compounds serving to dissolve them. Altogether, the production of these colloid solutions of substances, such as silica and alumina—in their crystalline state, as quartz and corundum, completely insoluble—threw an entirely new light upon the conditions of aqueous solution.

The colloidal solutions, obtained as above, of substances usually crystalline, were found to be exceedingly unstable. Either spontaneously, or on the addition of some or other crystalloid reagent, even in very minute quantity, they pectized or became converted into solid jellies. Hence Mr. Graham was led to speak of two colloidal states; the peptous, or dissolved, and the pectous, or gelatinized. In addition to their power of gelatinizing, their mutability, their non-crystalline habit, and their low diffusibility, substances in the colloid state were

found to be further characterized by their chemical inertness and by their high combining weights. Thus the saturating power of colloid silica was only about $\frac{1}{36}$th of that of the ordinary acid.

In his supplementary paper communicated to the Chemical Society, Mr. Graham showed how the pectous forms of different mineral colloids could, in many cases, be reconverted into their peptous forms. He further showed how the water of different peptous and pectous colloids could be mechanically displaced by other liquids, as alcohol, glycerine, sulphuric acid, &c. To the different classes of compounds, so formed, he gave distinctive names. Thus, the alcoholic solution and jelly, of silicic acid for instance, he designated as the alcosol and alcogel respectively.

Closely associated with the passage of different liquids through membranes is the action, known as endosmose, discovered by Dutrochet. Mr. Graham's principal results on this subject are recorded in a very elaborate paper "On Osmotic Force," communicated to the Royal Society in 1854;* but a few further results, and a statement of his final views, are contained in the paper, referred to immediately above, "On Liquid Diffusion applied to Analysis." When the solution of a saline or other compound is separated from an adjacent mass of water by a membranous septum, a greater or less quantity of the water very commonly passes through the septum into the solution; and if the solution be contained in a vessel of suitable construction, having a broad membranous base and a narrow upright stem, the water, in some cases, flows into the vessel through the membrane, with a force sufficient to raise and sustain a column of 20 inches or more of liquid in the stem. The problem is to account for this flow; which, with acid fluids more particularly, takes place in the reverse direction—*i. e.* from the solution into the water.

In the course of his experiments, Mr. Graham examined the osmotic movement produced with liquids of most diverse character; employing osmometers of animal membrane, albuminated calico, and baked earthenware. His results were, moreover, observed and recorded in very great detail. As an illustration of these results, it may be mentioned that with 1 per cent. solutions in the membranous osmometer, the liquid rose in the stem 2 millimètres in the case of common salt, 20 millimètres with chloride of calcium, 88 millimètres with chloride of nickel, 121 millimètres with chloride of mercury, 289 millimètres with proto-chloride of tin, 351 millimètres with chloride of copper, and 540 millimètres with chloride of aluminum. Mr. Graham showed, further, in opposition to the views of Dutrochet, that the velocity of the osmotic flow was not proportional to the quantity of salt or other substance originally contained in the solution; and that the flow did not depend on capillarity, as Dutrochet had inferred; or yet on diffusion, as some of his own experiments

* 'Phil. Trans.,' 1854, p. 177.

might be thought to indicate. Eventually he was led to the conclusion that osmose was essentially dependent on a chemical action taking place between one or other of the separated liquids and the material of the septum. He appears to have held somewhat different views of the nature of this chemical action at different times; and not to have considered it as being in all cases of the same character.

The following extracts, expressing his latest views on the subject, are taken from the conclusion of his paper, "On Liquid Diffusion applied to Analysis."

"It now appears to me that the water movement in osmose is an affair of hydration and of de-hydration in the substance of the membrane, or other colloid septum, and that the diffusion of the saline solution placed within the osmometer has little or nothing to do with the osmotic result otherwise than as it affects the state of hydration of the septum. . . . . Placed in pure water, such colloids (as animal membrane) are hydrated to a higher degree than they are in neutral saline solutions. Hence the equilibrium of hydration is different on the two sides of the membrane of an osmometer. The outer surface of the membrane being in contact with pure water, tends to hydrate itself in a higher degree than the inner surface does, the latter surface being supposed to be in contact with a saline solution. When the full hydration of the outer surface extends through the thickness of the membrane, and reaches the inner surface, it there receives a check. The degree of hydration is lowered, and water must be given up by the inner layer of the membrane, and it forms the osmose. . . . . Far from promoting this separation of water, the diffusion of the salt throughout the substance of the membrane appears to impede osmose by equalizing the condition as to saline matter of the membrane through its whole thickness. The advantage which colloidal solutions have in inducing osmose, appears to depend in part upon the low diffusibility of such solutions, and their want of power to penetrate the colloidal septum."

VI.

Movements of Gases under Pressure. Effusion and Transpiration.—The mechanical law of the passage of different gases under the same pressure through a mere perforation, as of the passage of different liquids, being that the velocities are inversely as the square roots of the specific gravities, Mr. Graham subjected this law to an experimental verification, and made known his results in a paper communicated to the Royal Society in 1846. The mode of experimenting was as follows:—A jar standing on the plate of an air-pump was kept vacuous by continued exhaustion, and a measured quantity of gas allowed to find its way into the jar through a minute aperture in a thin metallic plate. The admission of 60 cubic inches of dry air into the vacuous, or nearly vacuous jar, being arranged to take place in about 1000 seconds, the times of passage of the same volume of air were found not to vary from each other by more than two or three seconds in successive experiments.

Operating with different gases, the relative times of passage, or of "effusion," as it was denominated by Mr. Graham, proved to be approximatively identical with the square roots of the specific gravities of the several gases; or, in other words, their velocities of effusion were shown experimentally to be inversely as the square roots of their specific gravities. The rate of effusion of a mixed gas corresponded in most cases with the calculated mean rate of its constituents; but the rates of effusion of the light gases, marsh gas and hydrogen, were very disproportionately retarded by the admixture with them, even to a small extent, of the heavier gases, oxygen and nitrogen.

Passing from the study of the effusion of gases through a perforated plate, Mr. Graham next submitted their "transpiration" through a capillary tube to a similarly conducted experimental inquiry. His results were communicated to the Royal Society in two very elaborate papers, "On the Motion of Gases," Parts I. and II.,* the first part containing also his above-described results on the effusion of gases. With a very short capillary, the relative rates of passage of different gases were found to approximate to their relative rates of effusion; but with every elongation of the capillary, a constantly-increasing deviation from these rates was observed—the increase of the deviation, however, becoming less and less considerable with each successive increment of elongation, until, when the tube had acquired a certain length in proportion to its diameter, a maximum deviation of the relative rates of passage of the different gases from their relative rates of effusion was arrived at. These ultimate rates of passage, unaffected in relation to each other by further elongation of the capillary, constitute the true transpiration velocities of the different gases, as distinguished from their velocities of effusion. Of all the gases experimented on, oxygen was found to have the longest transpiration time, or slowest transpiration velocity. In the following Table its time of transpiration

	Specific Gravity.	$\sqrt{\text{Specific Gravity.}}$	Transpiration Time.
Hydrogen	·069	·263	·437
Marsh gas ..	·559	·747	·551
Nitrogen	·971	·985	·877
Oxygen	1·105	1·051	1·000
Carbonic gas ..	1·529	1·236	·730

is taken as unity, and the times of a few other gases compared therewith. In other columns are given the specific gravities of the same gases, referred to the specific gravity of air as unity; and the square roots of their specific gravities, which also express their relative times of effusion.

* 'Phil. Trans.,' 1846, p. 573; 1849, p. 349.

That gas transpiration has no direct relation to gas specific gravity is shown by the transpiration times of oxygen and nitrogen exceeding the transpiration times both of the much lighter hydrogen and marsh gas, and of the much heavier carbonic gas. Again, ammonia, olefiant gas, and cyanogen, with the different specific gravities ·590, ·978, and 1·806 respectively, have the almost identical transpiration times ·511, ·505, and ·506; or, approximatively, half the transpiration time of oxygen, 1·000. Nevertheless the transpiration times of oxygen and nitrogen are directly as their specific gravities; and further, the specific gravities of nitrogen, carbonic oxide, and nitric oxide being ·971, ·968, and 1·039, their transpiration times are ·877, ·874, and ·876 respectively. But then olefiant gas, with the same specific gravity ·978, has the much shorter transpiration time ·505; and similarly in other cases. Altogether the discordance between transpiration and specific gravity is of greater frequency than the accordance; but still the circumstance of gases having the same, or about the same, specific gravity, having also the same, or about the same, rate of transpiration, is of too frequent occurrence to be merely accidental.

As a rule, the observed transpiration rate of a mixture of gases corresponded with the calculated mean rate of its constituents; but the transpiration rates of the light gases, hydrogen and marsh gas, were found to be disproportionately retarded to a greater extent even than their effusion rates by the admixture with them of heavier gases. Further, by employing mixtures of gas and vapour, Mr. Graham extended his inquiry so as to include a determination of the transpiration times of several vapours; the results being calculated on the assumption that the observed transpiration time of the mixture was the mean of the transpiration times of the permanent gas and of the coercible vapour experimented on. In this way the transpiration time of ether vapour, sp. gr. 2·586, was shown to be identical with that of hydrogen gas, sp. gr. 0·069; and the transpiration time of carbonic sulphide vapour, sp. gr. 2·645, identical with that of sulphuretted hydrogen gas, sp. gr. 1·191.

With respect to gas transpiration in general, the rates of transpiration of different gases were found to be independent of the nature of the material of the capillary; apparently from the capillary, of what material soever, becoming lined with a film of gas, with which alone the current of gas could come in contact; so that the friction was purely intestine, and suggestive of a sort of viscosity in the gas itself. The rate of passage was further shown to be inversely as the length of the capillary; and directly, in some high but undetermined ratio, as its diameter. Lastly, the rate of "effusion" of a given volume of any particular gas being independent of pressure and temperature, the rate of transpiration of a given volume of any particular gas was observed to vary directly with its variation of density, whether the result of alteration of pressure or of temperature; 100 cubic inches of dense air, for example, transpiring more rapidly than 100 cubic inches of tenuous air, in proportion to the excess of density.

Speaking of the importance and fundamental nature of the physical properties manifested by bodies in the gaseous state, and of the extent of his own inquiries on gas-transpiration, Mr. Graham observed,—"It was under this impression that I devoted an amount of time and attention to that class of constants (transpiration-velocities) which might otherwise appear disproportionate to their value and the importance of the subject. As the results, too, were entirely novel, and wholly unprovided for in the received view of the gaseous constitution, of which indeed they prove the incompleteness, it was the more necessary to verify each fact with the greatest care."

VII.

Diffusion of Gases.—In 1801, Dalton, in an essay "On the Constitution of mixed Gases, and particularly of the Atmosphere," propounded the now celebrated view that "where two elastic fluids denoted by A and B are mixed together, there is no mutual repulsion amongst their particles; that is, the particles of A do not repel those of B, as they do one another; consequently the pressure or whole weight upon any one particle arises solely from those of its own kind." During the act of admixture, "the particles of A meeting with no repulsion from those of B . . . . would instantaneously recede from each other as far as possible under the circumstances, and consequently arrange themselves just as in a void space." At the beginning of 1803, in a supplementary paper "On the tendency of Elastic Fluids to Diffusion through each other," he made known the remarkable action of intermixture which takes place, even in opposition to the influence of gravity, when any two gases are allowed to communicate with each other. Thus, in a particular experiment, he showed that when a vial of hydrogen is connected with a vial of carbonic gas by means of a narrow piece of tubing, so that the vial of light hydrogen may be inverted over the other vial of heavy carbonic gas, the heavy carbonic gas actually ascends through the light hydrogen, and the light hydrogen descends through the heavy carbonic gas until the uniform admixture of the two gases with each other is effected. The subject was afterwards investigated by Berthelot, who, in a series of experiments performed with great care, while opposing Dalton's theoretical conclusions, corroborated his results; and indicated further the high diffusiveness of hydrogen. Here it was that Mr. Graham took up the inquiry. The first of his papers relating directly to the subject of gas-diffusion appeared in the 'Quarterly Journal of Science' for 1829, under the title, "A short Account of Experimental Researches on the Diffusion of Gases through each other, and their Separation by Mechanical Means."* The mode of proceeding adopted in these researches was as follows:—Each gas experimented on was allowed to diffuse from a horizontally placed bottle through a narrow tube,

* 'Quart. Journ. Sci.,' ii., 1829, p. 83.

directed either upwards or downwards according as the gas was heavier or lighter than air, so that the diffusion always had to take place in opposition to the influence of gravity. The result was that equal volumes of different gases escaped in very unequal times, the rapidity of the escape having an inverse relation to the specific gravity of the gas. Thus hydrogen was found to escape four or five times more quickly than the twenty-two times heavier carbonic gas. Again, with a mixture of two gases, the lightest or most diffusible of the two was found to leave the bottle in largest proportion, so that a sort of mechanical separation of gases could be effected by means of their unequal diffusibility. Most of these last results were obtained by allowing the gaseous mixture to diffuse into a limited atmosphere of some other gas or vapour, capable of subsequent removal by absorption or condensation.

But these methods of operating, by free or adiaphragmatic diffusion, were soon abandoned by Mr. Graham for the more practicable method of diffusion through porous septa. Once again, however, many years afterwards, in a paper "On the Molecular Mobility of Gases," to be more fully considered presently, Mr. Graham made some additional and very curious observations on the free diffusion of hydrogen and carbonic gas into surrounding air, showing the absolute velocities of the molecular movements in each of the two cases. A glass cylinder, ·57 mètre high, had the lowest tenth of its height filled with carbonic gas. Then, after different intervals of time, the uppermost tenth of air in the cylinder was drawn off and examined. In five minutes the carbonic gas in this upper tenth of air amounted to ·04, and in seven minutes to 1·02 per cent.; or 1 per cent. of carbonic gas had diffused to the distance of half-a-mètre in seven minutes, being at the rate of 73 millimètres per minute. Now, the conditions of this movement always prevail in the air of the atmosphere, and, using the words of Mr. Graham, "it is certainly most remarkable that in perfectly still air its molecules should spontaneously alter their position, and move to a distance of half-a-mètre in any direction in the course of five or six minutes." By similar experiments made with an inverted cylinder, 1 per cent. of hydrogen was found to diffuse downwards at the rate of 350 millimètres per minute, or about five times as rapidly as the carbonic gas diffused upwards.

With regard to Mr. Graham's experiments on the diffusion of gases through porous septa, his earliest results on this subject were communicated to the Royal Society of Edinburgh, in a paper "On the Law of the Diffusion of Gases," already referred to as the first-born of what may be considered his great papers.* Prior even to Dalton's above-mentioned experiments on free diffusion, Dr. Priestley, when transmitting different gases through stoneware tubes surrounded by burning fuel, perceived that the tubes were porous; and that not only was

* 'Edin. Roy. Soc. Trans.,' xii., 1834, p. 222.

there an escape of the gas, under pressure, from within the tube outwards to the fire, but that there was also a penetration of the exterior gases of the fire into the tube, notwithstanding the superior pressure of the current of gas passing through the tube.

Mr. Graham, however, appears to have had his attention originally directed to the study of the transmission of gases through porous diaphragms by the curious observation and experiments of Döbereiner, who, having occasion to collect and store some quantities of hydrogen over water, accidentally made use of a fissured jar, and was surprised to find that the water of the pneumatic trough rose in this jar to the height of an inch and a half in twelve hours, and to not far short of three inches in twenty-four hours. Having assured himself of the constancy of the phenomenon, Döbereiner attributed it to capillary action, conceiving hydrogen to be alone attractable by, and, on account of the assumed minuteness of its atoms, admissible through the fissure. In repeating Döbereiner's experiments, however, Mr. Graham soon observed that the escape of hydrogen outwards was always accompanied by a penetration of air inwards, the volume of air finding an entrance through the fissure amounting to about one-fourth of the volume of hydrogen making its escape; or the fissure proved permeable to the grosser air as well as to the finer hydrogen. Having arrived at this point, he replaced the fissured jar by an instrument admitting of much greater experimental precision. For the jar itself he substituted a piece of glass tube about half-an-inch in diameter and from eight to fourteen inches long, and for the fissure in the jar he substituted a plate of stucco serving to close one end of the tube. Operating with a diffusion-tube of this kind standing in a jar of water, it was found, as in Dalton's experiments, that the two gases, say external air and internal hydrogen, exhibited a powerful tendency to intermix or change places with each other; but more than this, it was found that the air did not exchange with its own volume of hydrogen, but instead with 3·8 times its volume. Using the word diffusion-volume to express the bulks of different gases exchanging thus with one another by the process of diffusion, the diffusion-volume of hydrogen would be 3·8, that of air being taken as 1. Similarly, it was ascertained that every gas has a diffusion-volume which is peculiar to itself, and is indeed inversely as the square root of its specific gravity; and since the unequal diffusion volumes of different gases are consequences of their unequal diffusion velocities, it follows that the relative velocities at which different gases diffuse into one another, by virtue of their own inherent mobility, are identical with those at which they effuse under pressure into a vacuum—a result quite in accordance with, and indeed deducible from, Dalton's aphorism. But although the relative rates of effusion and diffusion are alike, it is important, wrote Mr. Graham, in the later paper already quoted from, "to observe that the phenomena of effusion and diffusion are distinct and essentially different in their nature. The effusion movement affects masses of gas, the diffusion movement affects molecules; and a gas is

usually carried by the former kind of impulse with a velocity many thousand times as great as is demonstrated by the latter." *

Thus the result arrived at by Mr. Graham, in his original paper, was the enunciation of the now well-recognized law of the diffusion of gases; but, some thirty years afterwards, he again subjected the phenomena of gas-diffusion to an elaborate experimental investigation, —going over the old and penetrating into new ground with an activity by no means impaired, and with intellectual powers largely expanded by increase of years. His results were communicated to the Royal Society of London, in a paper "On the Molecular Mobility of Gases," † and it is impossible to read this and his original paper "On the Law of the Diffusion of Gases" together, without being struck by the great advance in philosophic grasp and breadth of view which had become developed in the long interval between the publication of the two memoirs. These later experiments on gas-diffusion were made principally with septa of compressed graphite; and it will be well to preface their consideration by Mr. Graham's own introductory remarks. He observes :—

"The pores of artificial graphite appear to be really so minute, that a gas *in mass* cannot penetrate the plate at all. It seems that molecules only can pass; and they may be supposed to pass wholly unimpeded by friction, for the smallest pores that can be imagined to exist in the graphite must be tunnels in magnitude to the ultimate atoms of a gaseous body. The sole motive agency appears to be that intestine movement of molecules which is now generally recognized as an essential property of the gaseous condition of matter.

"According to the physical hypothesis now generally received, a gas is represented as consisting of solid and perfectly elastic spherical particles or atoms, which move in all directions, and are animated with different degrees of velocity in different gases. Confined in a vessel, the moving particles are constantly impinging against its sides and occasionally against each other, and this contact takes place without any loss of motion, owing to the perfect elasticity of the particles. If the containing vessel be porous, like a diffusiometer, then gas is projected through the open channels, by the atomic motion described, and escapes. Simultaneously the external air is carried inwards in the same manner, and takes the place of the gas which leaves the vessel. To this atomic or molecular movement is due the elastic force, with the power to resist compression, possessed by gases. The molecular movement is accelerated by heat and retarded by cold, the tension of the gas being increased in the first instance and diminished in the second. Even when the same gas is present both within and without the vessel, or is in contact with both sides of our porous plate, the movement is sustained without abatement—molecules con-

* The motions of effusion under pressure, and of spontaneous diffusion, would appear to be alike traceable to the elasticity of the gas itself, exerted under the conditions to which it is exposed at the time.

† 'Phil. Trans.,' 1863, p. 385.

tinuing to enter and leave the vessel in equal number, although nothing of the kind is indicated by change of volume or otherwise. If the gases in communication be different, but possess sensibly the same specific gravity and molecular velocity, as nitrogen and carbonic oxide do, an interchange of molecules also takes place without any change in volume. With gases opposed of unequal density and molecular velocity, the permeation ceases of course to be equal in both directions."

One set of novel experiments recorded in the later paper, from which the above remarks are extracted, had reference to the diffusion of single gases through porous septa, into a vacuous or partially vacuous space. The diffusion-tube was substantially the same as that formerly employed, except in the circumstance of its being closed by a plate of compressed graphite instead of by stucco, and in the further circumstance of the tube itself being in some cases so far lengthened and otherwise modified as to admit of the production within it of a barometric vacuum of comparatively large dimensions. The mode of experimenting was as follows:—The short tubes, when employed, were filled with mercury, and inverted in a mercurial trough. Then, by means of a very simple arrangement, the gas under examination was allowed to sweep over the surface of, and diffuse through, the graphite plate, so as to depress the mercury within the tube until it stood at a height of 100 millimètres only,—that is, until the external pressure exceeded the internal pressure by 100 millimètres only. Matters being in this state, the experiment consisted in observing the number of seconds required for the admission through the graphite septum, into the graduated tube, of a given volume of gas—the mercury in the tube being kept throughout at the constant height of 100 millimètres, by a gradual lifting up of the tube, effected by a mechanical arrangement originally devised and employed by Professor Bunsen. The long tubes were filled with mercury in a different manner; but the conduct of the experiments made with them differed only from that of the experiments made with the short tubes, in that the level of mercury in the long tubes was maintained throughout at or near to the barometric height, so that the external gas diffused into the tube under full atmospheric pressure. Experimenting

	Times of Equal Diffusion.	Square Roots of Specific Gravities.
Oxygen	1·0	1·0
Air	·9501	·9507
Carbonic gas ..	1·1860	1·1760
Hydrogen	·2505	·2502

in this way, the relative times of permeation of equal volumes of different gases were found to be almost identical with the square roots

of the specific gravities of the respective gases, as shown in the preceding Table.

These results are of great value from the simplicity and constancy of the conditions under which they were obtained, and from their close accordance with the induced law. By allowing the diffusion to take place into a complete or partial vacuum, instead of into an atmosphere of other gas, the results were not complicated with those of interdiffusion; and by employing a thin plate of highly-compressed graphite, instead of a comparatively thick plug of more porous stucco, the results were not complicated with those of transpiration, as happened in some otherwise admirable experiments of Professor Bunsen, which led that distinguished investigator to question at one time the accuracy of Mr. Graham's law.

The absence of any transpiration of gas through the graphite wafer was made evident by the want of any approximation, in the rates of passage, to the characteristic rates of transpiration; and was consequent on the impermeability of the exceedingly minute pores of the graphite to any enforced bodily transmission of gas through them. It may be as well to state this conclusion in Mr. Graham's own words:—

"The movement of gases through the graphite plate appears to be solely due to their own proper molecular motion, quite unaided by transpiration. It seems to be the simplest possible exhibition of the molecular or diffusive movement of gases. This pure result is to be ascribed to the wonderfully fine (minute) porosity of the graphite. The interstitial spaces appear to be sufficiently small to extinguish capillary transpiration entirely. The graphite plate is a pneumatic sieve which stops all gaseous matter in mass, and permits molecules only to pass."

By similarly conducted experiments, a determination was also made of the difference of rate, if any, at which hydrogen diffuses through a graphite plate into a vacuum and into atmospheric air. Thus, in one minute of time, the following quantities of hydrogen passed through the graphite plate, in the two cases respectively:—

1·289 cubic centimètre into a vacuum.
1·243 cubic centimètre into air.

These numbers indicate a close approach to equality in the velocities of passage into a vacuum and into a space of other gas,—a yet closer equality being probably attainable by a modified form of experimenting.

The diffusion of hydrogen into air, as in the above-referred-to experiment, is of course accompanied by a diffusion of air into hydrogen, which had to be allowed for in calculating out the above result. Moreover, Mr. Graham made a special repetition of his early experiments on interdiffusion, operating with dry instead of moist gas, substituting mercury for water in the diffusion-tube, maintaining a constant pressure by Bunsen's mechanism instead of by

a pitcher of water, and using a wafer of graphite instead of a plug of stucco as the porous diaphragm. The theoretical exchange of hydrogen for air being 3·8 volumes for 1, and that of hydrogen for oxygen being 4·0 volumes for 1, the exchanging volumes actually found were 3·876 and 4·124 respectively.

Referring to the approximatively equally rapid passage of hydrogen into a vacuous and an aërial space, Mr. Graham remarks as follows on the subject of interdiffusion :—

"In fine, there can be little doubt left on the mind that the permeation through the graphite plate into a vacuum, and the diffusion into a gaseous atmosphere, through the same plate, are due to the same inherent mobility of the gaseous molecule. They are the exhibition of this movement in different circumstances. In interdiffusion we have two gases moved simultaneously through the passages in opposite directions, each gas under the influence of its own inherent force ; while with gas on one side of the plate, and a vacuum on the other side, we have a single gas moving in one direction only. The latter case may be assimilated to the former if the vacuum be supposed to represent an infinitely light gas. It will not involve any error, therefore, to speak of both movements as gaseous diffusion,—the diffusion of gas into gas (double diffusion) in the one case, and the diffusion of gas into a vacuum (single diffusion) in the other. The inherent molecular mobility may also be justly spoken of as the diffusibility or diffusive force of gases.

"The diffusive mobility of the gaseous molecule is a property of matter, fundamental in its nature, and the source of many others. The rate of diffusibility of any gas has been said to be regulated by its specific gravity, the velocity of diffusion having been observed to vary inversely as the square root of the density of the gas. This is true, but not in the sense of the diffusibility being determined or *caused* by specific gravity. The physical basis is the molecular mobility. The degree of motion which the molecule possesses regulates the volume which the gas assumes, and is obviously one, if not the only, determining cause of the peculiar specific gravity which the gas enjoys. If it were possible to increase in a permanent manner the molecular motion of a gas, its specific gravity would be altered, and it would become a lighter gas. With the density is also associated the equivalent weight of a gaseous element, according to the doctrine of equal combining volumes."

In addition to the above two sets of experiments, on the diffusion of a single gas into a vacuum and on the diffusion of one gas into another, a third set of experiments was made on the diffusion of one gas away from another; or on the partial separation of mixed gases by the process of atmolysis. The experiments on this subject were conducted in several different ways, but the most striking results were obtained with what Mr. Graham named his tube atmolyser. This instrument consists of one or more lengths of ordinary tobacco-pipe (conveying the current of mixed gas), surrounded by a glass tube

maintained in a more or less vacuous state by exhaustion with an air-pump. The most diffusible constituent of the mixed gas passing away in largest proportion through the porous material of the tobacco-pipe, the least diffusible constituent becomes concentrated in the residue of gas passing along, and finally delivered by the pipe. By this simple contrivance, the proportion of oxygen in ordinary air, transmitted by the tobacco-pipe, was increased from below 21 up to 24·5 per cent., as a result of the small superior diffusive velocity of nitrogen 1·01, over that of oxygen 0·95.

In experiments made with the far more unequally diffusive gases oxygen and hydrogen, mixed in equal volumes, the proportion of oxygen transmitted by the tobacco-pipe was increased from the original 50 per cent., to 90, and even in some cases to 95 per cent. Electrolytic gas, consisting of 33·3 per cent. oxygen and 66·6 per cent. hydrogen, was slowly transmitted through a single tobacco-pipe, in some experiments enclosed in a vacuum, in others exposed to the air. In the vacuum experiments the transmitted gas was found to consist of 90·7 per cent. oxygen and 9·3 per cent. hydrogen. In the air experiments, the transmitted gas was found to consist of 40·4 per cent. oxygen, 5·5 per cent. hydrogen, and 54·1 per cent. air. In both cases it had lost its explosive character, and acquired the property of re-inflaming a glowing splinter.

This paper of Mr. Graham's "On the Molecular Mobility of Gases" was supplemented by a communication made to the Chemical Society, in 1864, entitled "Speculative Ideas respecting the Constitution of Matter,"* from which the following extracts are taken:—

"It is conceivable that the various kinds of matter, now recognized as different elementary substances, may possess one and the same ultimate or atomic molecule existing in different conditions of movement. The essential unity of matter is an hypothesis in harmony with the equal action of gravity upon all bodies. We know the anxiety with which this point was investigated by Newton, and the care he took to ascertain that every kind of substance, 'metals, stones, woods, grain, salts, animal substances,' &c., are similarly accelerated in falling, and are therefore equally heavy.

"In the condition of gas, matter is deprived of numerous and varying properties, with which it appears invested when in the form of a liquid or solid. The gas exhibits only a few grand and simple features. These again may all be dependent upon atomic or molecular mobility. Let us imagine one kind of substance only to exist—ponderable matter; and further, that matter is divisible into ultimate atoms, uniform in size and weight. We shall then have one substance and a common atom. With the atom at rest the uniformity of matter would be perfect. But the atom possesses always more or less motion, due, it must be assumed, to a primordial impulse. This motion gives rise to volume. The more rapid the movement the greater the space

* 'Chem. Soc. Journ.,' xvii., p. 368.

occupied by the atom, somewhat as the orbit of a planet widens with the degree of projectile velocity. Matter is thus made to differ only in being lighter or denser matter. The specific motion of an atom being inalienable, light matter is no longer convertible into heavy matter. In short, matter of different density forms different substances—different inconvertible elements, as they have been considered.

"But further, these more and less mobile, or light and heavy forms of matter, have a singular relation connected with equality of volume. Equal volumes of two of them can coalesce together, unite their movement, and form a new atomic group, retaining the whole, the half, or some simple proportion of the original movement and consequent volume. This is chemical combination. It is directly an affair of volume, and only indirectly connected with weight. Combining weights are different, because the densities, atomic and molecular, are different. The volume of combination is uniform, but the fluids measured vary in density. This fixed combining measure—the *metron* of simple substances—weighs 1 for hydrogen, 16 for oxygen, and so on with the other 'elements.'

"To the preceding statements respecting atomic and molecular mobility, it remains to be added that the hypothesis admits of another expression. As in the theory of light we have the alternative hypotheses of emission and undulation, so in molecular mobility the motion may be assumed to reside either in separate atoms and molecules, or in a fluid medium caused to undulate. A special rate of vibration or pulsation originally imparted to a portion of the fluid medium enlivens that portion of matter with an individual existence, and constitutes it a distinct substance or element.

"Lastly, molecular or diffusive mobility has an obvious bearing upon the communication of heat to gases by contact with liquid or solid surfaces. The impact of the gaseous molecule, upon a surface possessing a different temperature, appears to be the condition for the transference of heat, or the heat movement, from one to the other. The more rapid the molecular movement of the gas, the more frequent the contact with consequent communication of heat. Hence, probably, the great cooling power of hydrogen gas as compared with air or oxygen. The gases named have the same specific heat for equal volumes, but a hot object placed in hydrogen is really *touched* 3·8 times more frequently than it would be if placed in air, and 4 times more frequently than it would be if placed in an atmosphere of oxygen gas. Dalton had already ascribed this peculiarity of hydrogen to the high 'mobility' of that gas. The same molecular property of hydrogen recommends the application of that gas in the air-engine, where the object is to alternately heat and cool a confined volume of gas with rapidity."

VIII.

Passage of Gases through Colloid Septa.—In 1830, Dr. Mitchell, of Philadelphia, discovered a power in gases to penetrate thin sheet

india-rubber; and, noticing the comparatively rapid transmission of carbonic gas through the rubber, associated this observation with the further one that a solid piece of india-rubber is capable of absorbing its own volume of carbonic gas, when left in contact with excess of the gas for a sufficient length of time. By means of a suitable arrangement, Dr. Mitchell found that various gases passed spontaneously through a caoutchouc membrane into an atmosphere of ordinary air with different degrees of velocity—that as much of ammonia gas was transmitted in 1 minute as of carbonic gas in 5½ minutes, as of hydrogen in 37½ minutes, and as of oxygen in 113 minutes. Soon after their publication, these results were ably commented on and extended by Dr. Draper, of New York; and, altogether, they attracted considerable attention in scientific circles. One of Mr. Graham's earliest observations—having reference to the spontaneous passage of carbonic gas into a moist bladder of air, so as ultimately to burst the bladder—had obviously a very close connection with Dr. Mitchell's results, and received from Mr. Graham in 1829 the same explanation that in 1866 he gave to his own india-rubber experiments, the account of which he communicated to the Royal Society in a paper "On the Absorption and Dialytic Separation of Gases by Colloid Septa." * In his experiments on the penetration of different gases, through septa of india-rubber, into a vacuum, Mr. Graham employed a tube considerably exceeding in length the barometric column, open at one end and closed at the other by a thin film of caoutchouc stretched over a plate of highly porous stucco. On filling this tube with mercury, and inverting it into a cup of mercury, a Torricellian vacuum was left at the top, into which the external air, or any external gas experimented on, gradually found its way by passage through the caoutchouc film, so as to cause a depression of the mercurial column. By experiments made in this manner, it was found that different gases penetrated the rubber, and entered the vacuous space with the following relative velocities, differing widely from the velocities of diffusion and transpiration of the same gases given in the other two columns of the Table:—

	Rates of Passage through Caoutchouc.	Transpiration Velocities.	Diffusion Velocities.
Nitrogen	1·00	1·14	1·01
Marsh gas ..	2·15	1·81	1·34
Oxygen	2·55	1·00	·95
Hydrogen	5·50	2·29	3·80
Carbonic gas ..	13·58	1·37	·81

Bearing in mind the partial separation of gases from one another attainable by reason of their unequal diffusive velocities, the pos-

* 'Phil. Trans.,' 1866, p. 399.

sibility of effecting a similar separation of gases by reason of their unequal velocities of transmission through india-rubber was easily to be foreseen. For example, atmospheric air consisting of 20·8 volumes of oxygen and 79·2 volumes of nitrogen, and the transmission velocities of these two gases being respectively 2·55 and 1·0, it follows that the air transmitted through india-rubber into a vacuum should consist of 40 per cent. oxygen and 60 per cent. nitrogen, thus:—

$$\left.\begin{array}{lll} \text{Oxygen} & .. & 20{\cdot}8 \times 2{\cdot}55 = 53{\cdot}04 \\ \text{Nitrogen} & .. & 79{\cdot}2 \times 1{\cdot}0 = 79{\cdot}20 \\ & & \overline{132{\cdot}24} \end{array}\right\} \text{ or } \left\{\begin{array}{l} 40 \\ 60 \\ \overline{100} \end{array}\right.$$

In subjecting this conclusion to the test of experiment, Mr. Graham availed himself of Dr. Sprengel's then newly-invented mercurial pump or exhauster, an instrument which also stood him in good stead in his subsequent work, and to which he freely acknowledged his obligations. By a slight alteration in the pump, as originally constructed, Mr. Graham made it serve not only for its original purpose of creating and maintaining an almost perfect vacuum, but also for delivering *pari passu* any gas penetrating into the vacuum through its caoutchouc or other walls.

The caoutchouc films employed in these experiments were of various kinds; but the most readily practicable and, on the whole, successful results, were obtained with india-rubber varnished silk made up into a flat bag, exposing on each side about 0·25 mètre squared of surface. The interior of such a bag being in communication with the Sprengel pump, the constituents of the external air were gradually sucked through the walls of the bag and delivered by the turned-up fall-tube of the pump. On examining the delivered gas, it was found to contain on the average 41·6 per cent. of oxygen; and accordingly, to have the property of re-inflaming a glowing splinter. Thus, by the simple suction of atmospheric air through a caoutchouc film, the remarkable result was arrived at of nearly doubling the proportion of oxygen in the volume of air sucked through. Unfortunately for the practical application of the process, the entire volume of air sucked through proved to be very small, about 2·25 cubic centimètres per minute, per square mètre of surface, at 20° C. At 60° C., however, the passage of air through the rubber was almost exactly three times as rapid as at 20°.

Instead of allowing the gases experimented on to pass through the india-rubber into a vacuous space, they were in some cases allowed to pass into a space already occupied with a different gas, somewhat as in Dr. Mitchell's original experiments; but the conditions of the action were then more complex. The constituent gases of atmospheric air, for instance, pass through an india-rubber septum into a space containing carbonic gas at the relative velocities with which they enter a vacuous space; but throughout the experiment, not only are oxygen and nitrogen continually entering the space, but carbonic gas

is continually, and very rapidly, escaping from it. Eventually, by the rapid escape of carbonic gas, the proportion or pressure of oxygen in the internal space comes to exceed that in the external air; whereupon a reverse transmission, through the india-rubber, of the excess of oxygen into the external air, at once begins. But by stopping the operation at an early stage, and then absorbing the carbonic gas with caustic alkali, a residue of hyperoxygenized air was left, capable, in some cases, of re-inflaming a glowing splinter, and containing as much as 37·1 volumes of oxygen to 62·9 volumes of nitrogen.

The interpretation, given by their discoverer to the above results, was in accordance with his slowly developed views on the relations of gases and liquids to each other and to soft solids. Having satisfied himself that the merest film of india-rubber is quite devoid of porosity, and that oxygen is at least twice as absorbable by india-rubber as by water at ordinary temperature (the absorbability of its own volume of carbonic gas by india-rubber, as by water, having been noticed by Dr. Mitchell), Mr. Graham came to view the entire phenomenon as having a very complex character,—as consisting in a dissolution of the gas in the soft india-rubber; in a diffusion of the liquefied gas, as a liquid, through the thickness of the india-rubber; in an evaporation of the liquefied gas from the internal surface of the india-rubber; and lastly in a diffusion of the evaporated gas into the internal space. Thus, in reference to the remarks of Drs. Mitchell and Draper, he writes—"These early speculations lose much of their fitness from not taking into account the two considerations already alluded to, which appear to be essential to the full comprehension of the phenomenon—namely, that gases undergo liquefaction when absorbed by liquids and such colloid substances as india-rubber, and that their transmission through liquid and colloid septa is then effected by the agency of liquid and not gaseous diffusion. Indeed, the complete suspension of the gaseous function during the transit through colloid membrane cannot be kept too much in view." Mr. Graham seems thus to have recognized at least three distinct modes of gas transmission through a solid or semi-solid septum:—

1st. By a sufficient degree of pressure gases might be forced bodily, *i. e.* in masses, through the minute channels of a porous septum; or, in other words, might pass through such a septum by *transpiration*, of course in the direction only of the preponderating total pressure.

2nd. As the channels of a porous septum became more and more minute, their resistance to the bodily transmission of gas would become greater and greater, and the quantity of gas forced through them less and less, until at length the septum would be absolutely impermeable to transpiration under the particular pressure. But such a septum, of which the individual capillary channels were so small as to offer a frictional resistance to the passage of gas greater than the available pressure could overcome, might nevertheless present a considerable aggregate of interspace through which the *diffusion* proper

of gases, consequent on their innate molecular mobility, could take place freely in both directions.

3rd. A septum might be quite free from pores, of any kind or degree of minuteness, and so far be absolutely impermeable to the transmission of gas in the form of gas; but it might nevertheless permit a considerable transmission of certain gases by reason of their prior solution or *liquefaction* in the substance of the septum. And whereas the mere passage of gas, by transpiration or diffusion through a porous septum, would take place in thorough independence of the nature of the material of the septum, in this last considered action, the transmission would take place by virtue of a sort of chemical affinity between the gas and the material of the septum—the selective absorption of the gas by the septum being a necessary antecedent of its transmission; whence it might be said the gas was transmitted because it was first absorbed. Of course in certain transmissions two, or all three, modes of action might come into play simultaneously.

IX.

Occlusion of Gases by Metals.—The experiments of Deville and Troost having made known the curious fact of the permeability of ignited homogeneous platinum and ignited homogeneous iron to hydrogen gas, and given some indication also of the permeability of ignited iron to carbonic oxide gas, Mr. Graham, in 1866, corroborated the results of the French chemists in reference to platinum; but, modifying their method by letting the hydrogen pass into a space kept vacuous by the Sprengel pump, instead of into an atmosphere of other gas, assimilated the process to that which he had employed in his india-rubber experiments. The results he obtained were communicated to the Royal Society, partly in the paper already referred to "On the Absorption and Separation of Gases by Colloid Septa," and partly in four supplementary notices published in the Proceedings of the Society.* In carrying out the investigation forming the subject of these several communications, Mr. Graham had the advantage of being admirably seconded by his assistant, Mr. W. Chandler Roberts, whose able and zealous co-operation he repeatedly acknowledged in the warmest terms.

In the course of experiments made on the transmission of gases through ignited metallic septa, a particular platinum tube, being rendered vacuous, was found at all temperatures below redness to be quite impermeable to hydrogen; whereas at a red heat, it transmitted 100 cubic centimètres of hydrogen in half-an-hour—the quantities of oxygen, nitrogen, marsh gas, and carbonic gas, transmitted under the same conditions, not amounting to ·01 cubic centimètre each in half-an-hour. It was ascertained further that, with an ignited vacuous tube of platinum surrounded by a current of ordinary coal gas (a variable

* 'Roy. Soc. Proc.,' xv., p. 502; xvi., p. 422; xvii., p. 212, p. 500.

mixture of gases containing on the average about 45 per cent. of marsh gas, 40 per cent. of hydrogen, and 15 per cent. of other gases and vapours), a transmission of pure hydrogen alone took place through the heated metal. This property of selective transmission, manifested by platinum, was so far analogous to the property of selective transmission manifested by india-rubber, that whereas a septum of india-rubber transmitted the nitrogen of the air in a much smaller ratio than the oxygen, the septum of ignited platinum transmitted the other constituents of coal gas in an infinitely smaller ratio than the hydrogen. Hence the knowledge, of the absorption by india-rubber of the gases which it most freely transmitted, suggested to Mr. Graham an inquiry as to the possible absorption of hydrogen gas by platinum. Accordingly platinum, in different forms, was heated to redness, and then allowed to cool slowly in a continuous current of hydrogen. The metal so treated, and after its free exposure to the air, was placed in a porcelain tube, which was next made vacuous by the Sprengel pump. During the production and maintenance of the vacuum, no hydrogen was extracted from the metal at ordinary temperatures; or even during an hour's exposure to the temperature of 220°; or yet at a heat falling just short of redness. But at a dull red-heat and upwards, a quantity of hydrogen gas was given off amounting in volume, measured cold, to as much, in some cases, as 5·5 times the volume of the platinum. Thus was opened out to Mr. Graham the subject of his last, and probably greatest discovery—the occlusion of gases by metals. Very many metals were examined in their relations to different gases, but the most interesting results were those obtained with platinum as above described; and those obtained with silver, with iron, and above all, with palladium.

The characteristic property of silver, heated and cooled in different atmospheres, proved to be its capability of absorbing and retaining, in some cases, as much as seven times its volume of oxygen—its absorption of hydrogen falling short of a single volume. Some silver leaf, heated and cooled in ordinary air, and subsequently heated in a vacuum, gave off a mixture of oxygen and nitrogen gases containing 85 per cent. of oxygen, or more than four times the proportion contained in the original air. This remarkable property of solid silver to effect the permanent occlusion of oxygen gas, must be distinguished from the not less remarkable and doubtless associated property of melted silver to effect the temporary absorption of a yet larger volume of the same gas; which, on the solidification of the metal, is discharged with the well-known phenomenon of spitting.

Iron, though tolerably absorptive of hydrogen, was found to be specially characterized by its absorption of carbonic oxide. What may be called the natural gas of wrought iron, or the gas derived from the forge in which it was heated, proved to consist chiefly of carbonic oxide, and, in different experiments, was found to range from 7 to 12·5 times the volume of the metal; so that, in the course of its preparation, iron would appear to occlude upwards of seven times its

volume of carbonic oxide, and to carry this gas about with it ever after. The absorbability of carbonic oxide by iron has an obviously important bearing on the theory of steel production by cementation. This process would appear to consist in an absorption of carbonic oxide gas into the substance of the iron, and in a subsequent decomposition of the absorbed gas into carbon entering into combination with the metal, so as to effect its acieration, and carbonic gas discharged from the surface of the metal, so as to produce the well-known appearance of blistering. Nor is this the only, or even the chief point of interest that was made out with regard to iron; for the study of the behaviour of telluric manufactured iron naturally led Mr. Graham to the examination of sidereal native iron, that is to say, the iron of meteorites, and with the following result. A portion of meteoric iron, from the Lenarto fall, when heated in vacuo, gave off 2·85 times its volume of natural gas, of which the preponderating constituent, to the extent of 85·7 per cent. of the total quantity, consisted not of carbonic oxide, but of hydrogen, the carbonic oxide amounting to only 4·5 per cent., and the remaining 9·8 per cent. consisting of nitrogen. The inference that the meteorite had been, at some time or other, ignited in an atmosphere having hydrogen as its prevailing constituent, seems irresistible; and judging from the volume of gas yielded by the iron, the hydrogen atmosphere in which it was ignited must, in all probability, have been a highly condensed one; the charge of hydrogen extracted being fully five times as great as it was found possible to impart to ordinary iron artificially.

But it was with palladium that Mr. Graham obtained his most extraordinary results. This metal he found to have the property of transmitting hydrogen with extreme facility, even at temperatures very far short of redness. Coincidently, at temperatures even below those requisite for transmission, palladium was found capable of absorbing many hundred times its volume of hydrogen. Thus a piece of palladium foil maintained at a temperature of 90°–97° for three hours, and then allowed to cool down during an hour and a half, while surrounded by a continuous current of hydrogen gas, gave off, on being afterwards heated in vacuo, 643 times its volume of the gas, measured cold; and even at ordinary temperatures, it absorbed 376 times its volume of the gas, provided it had first been recently ignited in vacuo. In another experiment, palladium sponge, heated to 200° in a current of hydrogen and allowed to cool slowly therein, afterwards yielded 686 times its volume of the gas; while a piece of electrolytically deposited palladium heated only to 100° in hydrogen, afterwards yielded, upon ignition in vacuo, no less than 982 times its volume of the gas. The lowness of the temperature at which, under favourable circumstances, the absorption of hydrogen by palladium could thus be effected, soon suggested other means of bringing about the result. For example, a piece of palladium foil was placed in contact with a quantity of zinc undergoing solution in dilute sulphuric acid; and, on subsequent examination, was found to have absorbed 173 times its volume of hydrogen. Again, palla-

dium, in the forms of wire and foil, was made to act as the negative pole of a Bunsen's battery effecting the electrolysis of acidulated water; and in this manner was found to absorb from 800 to 950 times its volume of hydrogen in different experiments.

Palladium being thus chargeable with hydrogen in three different ways, namely, by being heated and cooled in an atmosphere of the gas; by being placed in contact with zinc dissolving in acid, *i. e.* with hydrogen in the act of evolution; and, lastly, by being made the negative electrode of a battery,—correlatively, the charged metal could be freed from its occluded hydrogen by exposing it to an increase of temperature in air or vacuo; by acting on it with different feebly oxidizing mixtures; and by making it the positive electrode of a battery.

The palladium, when charged to its maximum, was frequently found to give off a small proportion of its hydrogen, though with extreme slowness, at ordinary temperatures, both into the atmosphere and into a vacuum. But not until the temperature approached 100° was there any appreciable gas-evolution; which, above that point, took place with a facility increasing with the temperature, so as to be both rapid and complete at about 300°. Since, however, the transmission of hydrogen through heated palladium is a phenomenon of simultaneous absorption and evolution, it follows that the property of palladium to absorb hydrogen does not cease at 300°, or indeed at close upon the melting-point of gold,—the highest temperature at which Mr. Graham's experiments on transmission were conducted; but whereas the maximum absorption of hydrogen by palladium takes place at comparatively low temperatures, the velocity of transmission was observed to increase, in a rapid ratio, with the increase of temperature, indefinitely.

As regards the removal of hydrogen from palladium by oxygenants, the gas of the charged metal was found to manifest all the chemical activity of hydrogen in the nascent state. Thus it reduced corrosive sublimate to calomel, combined directly with free iodine, converted ferrid- into ferro-cyanides, destroyed the colour of permanganates, &c. Moreover, the spongy metal, charged with hydrogen and exposed to the air, was apt to become suddenly hot, and so completely discharged, by a spontaneous aërial oxidation of its absorbed gas into water; while the hydrogen of a piece of charged palladium wire was often capable of being set fire to, and of burning continuously along the wire.

Lastly, the reversal of the position of the palladium plate in the decomposing cell of the battery afforded a most ready means of completely extracting its hydrogen. Indeed, for some time after the reversal, while hydrogen was being freely evolved from the negative pole, no oxygen was observable on the surface of the palladium plate, now made the positive pole, through its rapid oxygenation of the absorbed hydrogen.

As regards the extent of the absorption of hydrogen by palladium, it was found, as already indicated, to vary considerably with the phy-

sical state of the metal, whether fused, hammered, spongy, or electrolytically deposited, for example. In one case, previously referred to, a specimen of electrolytically deposited palladium, heated to 100°, and then slowly cooled in a continuous current of hydrogen, was found to occlude 982·14 times its volume of the gas, measured cold. In this case, the actual weight of palladium experimented with, was 1·0020 gramme, and the weight of hydrogen absorbed ·0073 gramme, being in the ratio of 99·277 per cent. of palladium and 0·723 per cent. of hydrogen. The atomic weight of hydrogen being 1, and that of palladium 106·5, it is observable that the ratio of the weights of the constituents of the charged metal, hydrogen and palladium, approximates to the ratios of their atomic weights.

In another experiment some palladium wire, drawn from a piece of the fused metal, was charged electrolytically with 935·67 times its volume of hydrogen. Some idea of these enormous absorptions of hydrogen may be formed by remembering that water at mean temperature absorbs only 782·7 times its volume of that most absorbable of the common gases, ammonia.

A point of interest with regard to the different quantities of hydrogen absorbable by palladium in its different states, is the gradual diminution in the absorptive power of any particular specimen of the metal, with each successive charge and discharge of gas, in whatever way effected—the absorptive power, however, being partially restorable by subjecting the metal to a welding heat.

The density of palladium charged with eight or nine hundred times its volume of hydrogen is perceptibly lowered. Owing, however, to a continuous formation of bubbles of hydrogen on the surface of the charged metal when immersed in water, there is a difficulty in taking its exact density by comparing its respective weights in air and water with one another. There is also a difficulty in determining the density by direct measurement of the charged palladium when in the form of wire; owing to the curious property of the wire, on being discharged, of not merely returning to its original volume, but of undergoing a considerable and permanent additional retraction. But in the case of certain alloys of platinum, silver, and gold with excess of palladium, while the absorptive power of the constituent palladium is still manifested, the excess of retraction on discharge of the wires does not occur; and the specific gravities deducible from the mere increase in length of wires of these alloys, are found to accord approximatively with those deducible from the increase in length of the pure palladium wire, not above its original length, but above the length to which it retracts on discharge of its absorbed gas. It would thus appear that, simultaneously with its absorption of hydrogen, the pure palladium wire, unstably stretched by the process of drawing, suffers two opposite actions; that is to say, it undergoes a process of shortening by assuming a more stable condition of cohesion, and a process of lengthening by the addition to it of other matter—the lengthening due to the additional matter being the excess of the length of the charged above

that of the discharged wire. In a particular experiment illustrative of this peculiarity, a new platinum wire took up a full charge of hydrogen electrolytically, namely, 956·3 volumes, and increased in length from 609·585 to 619·354 millimètres. With the expulsion of the hydrogen afterwards, the wire was permanently shortened to 600·115 millimètres. The sum of the two changes taken together amounts to 19·239 millimètres, and represents the true increase in the length of the wire due to the addition of hydrogen. It corresponds to a linear expansion of 3·205 in 100, or to a cubical expansion of 9·827 in 100. The original volume of the wire being ·126 cubic centimètre, the volume of the condensed hydrogen would accordingly be ·01238 cubic centimètre. Then as the charged wire, on being heated in vacuo, evolved 120·5 cubic centimètres of hydrogen gas, weighing ·0108 gramme, the density of the absorbed hydrogen would be

$$\frac{\cdot 01080}{\cdot 01238} = \cdot 872.$$

Calculated from the mere increase in length of the charged wire above that of the wire originally, the density of the absorbed hydrogen would be 1·708. The following Table gives the densities of condensed hydrogen in different experiments made with palladium wire, in which the excess of retraction on discharge was allowed for as above; and also the densities observed in experiments made with palladium alloys in which the contraction on discharge took place to the original lengths of the wires only.

When united with	Density of Condensed Hydrogen.
Palladium	0·854 to 0·872
Palladium and platinum	0·7401 „ 0·7545
Palladium and gold	0·711 „ 0·715
Palladium and silver	0·727 „ 0·742

Whether the absorption of hydrogen by palladium, alloyed or not with another metal, was large or small, the density of the occluded hydrogen was found to be substantially the same. That the excessive retraction of the palladium wire on the discharge of its absorbed hydrogen is not a mere effect of heat was shown by the charged wire undergoing a similar retraction when discharged electrolytically instead of by ignition in vacuo; and also by the original wire not undergoing any sensible retraction as a result of annealing. That the retraction is merely in length was shown by the absence of any difference in specific gravity between the original and the discharged wire. Very curiously, the shortening of the wire, by successive chargings and dischargings of hydrogen, would seem to be interminable. Thus the following expansions of a particular wire, caused by variable charges of hydrogen, were followed, on expelling the hydrogen, by the contractions recorded in the other column.

	Elongation in Millimètres.	Retraction in Millimètres.
1st Experiment	9·77	9·70
2nd „	5·765	6·20
3rd „	2·36	3·14
4th „	3·482	4·95
		23·99

The palladium wire, which originally measured 609·144 millimètres, thus suffered, by four successive chargings and dischargings of hydrogen, an ultimate contraction of 23·99 millimètres, or a reduction of its original length to the extent of nearly 4 per cent., each increment of contraction below the original length usually exceeding the previous increment of elongation above the original length of the wire. The alternate expansion and contraction of palladium by its occlusion and evolution of hydrogen is ingeniously shown by a contrivance of Mr. Roberts', in which a slip of palladium foil, varnished on one side, is made to curl and uncurl itself, as it becomes alternately the negative and positive electrode of a battery, or is alternately charged and discharged of hydrogen on its free surface.

That hydrogen is the vapour of a highly volatile metal has frequently been maintained on chemical grounds; and from a consideration of the physical properties of his hydrogenized palladium, Mr. Graham was led to regard it as a true alloy of palladium with hydrogen, or rather hydrogenium, in which the volatility of the latter metal was restrained by the fixity of the former, and of which the metallic aspect was equally due to both of its constituents. Although, indeed, the occlusion of upwards of 900 times its volume of hydrogen was found to lower the tenacity and electric conductivity of palladium appreciably, still the hydrogenized palladium remained possessed of a most characteristically metallic tenacity and conductivity. Thus, the tenacity of the original wire being taken as 100, the tenacity of the fully charged wire was found to be 81·29; and the electric conductivity of the original wire being 8·10, that of the hydrogenized wire was found to be 5·99. In further support of the conclusion arrived at by Mr. Graham, as to the metallic condition of the hydrogen occluded in palladium, he adduced his singular discovery of its being possessed of magnetic properties, more decided than those of palladium itself, a metal which Mr. Faraday had shown to be "feebly but truly magnetic." Operating with an electromagnet of very moderate strength, Mr. Graham found that while an oblong fragment of electrolytically deposited palladium was deflected from the equatorial by 10° only, the same fragment of metal, charged with 604·6 times its volume of hydrogen, was deflected through 48°. Thus did Mr. Graham supplement the idea of hydrogen as an invisible incondensable gas, by the idea of hydrogen as an opaque, lustrous, white metal, having a specific gravity between 0·7 and 0·8, a well-marked tenacity and conductivity, and a very decided magnetism.

[W. O.]

WEEKLY EVENING MEETING,

Friday, February 4, 1870.

WILLIAM SPOTTISWOODE, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

JOHN RUSKIN, Esq. F.R.S. M.R.I.
SLADE PROFESSOR OF FINE ARTS, OXFORD,

A Talk respecting Verona and its Rivers.

THE discourse began with a description of the scenery of the eastern approach to Verona, with special remarks upon its magnificent fortifications, consisting of a steep ditch, some thirty feet deep by sixty or eighty wide, cut out of the solid rock, and the precipice-like wall above, with towers crested with forked battlements set along it at due intervals. The rock is a soft and crumbling limestone, containing "fossil creatures still so like the creatures they were once, that there it first occurred to the human brain to imagine that the buried shapes were not mockeries of life, but had indeed once lived; and, under those white banks by the road-side was born, like a poor Italian gipsy, the modern science of geology—" . . . "The wall was chiefly built, the moat entirely excavated, by Can Grande della Scala; and it represents typically the form of defence which rendered it possible for the life and the arts of citizens to be preserved and practised in an age of habitual war. Not only so, but it is the wall of the actual city which headed the great Lombard league, which was the beginner of personal and independent power in the Italian nation, and the first banner-bearer, therefore, of all that has been vitally independent in religion and in art throughout the entire Christian world to this day." At the upper angle of the wall, looking down the northern descent, is seen a great round tower at the foot of it, not forked in battlements, but with embrasures for guns. "The battlemented wall was the cradle of civic life. That low circular tower is the cradle of modern war and of all its desolation. It is the first European tower for artillery; the beginning of fortification against gunpowder—the beginning, that is to say, of the end of *all* fortification."

After noticing the beautiful vegetation of the district, Mr. Ruskin described the view from the promontory or spur, about ten miles long, of which the last rock dies into the plain at the eastern gate of Verona. "This promontory," he said, "is one of the sides of the great gate out of Germany into Italy, through which the Goths always entered; cloven up to Innspruck by the Inn, and down to Verona by the

Adige. And by this gate not only the Gothic armies came, but after the Italian nation is formed the current of northern life enters still into its heart through the mountain artery, as constantly and strongly as the cold waves of the Adige itself." . . . "The rock of this promontory hardens as we trace it back to the Alps, first into a limestone having knots of splendid brown jasper in it as our chalk has flints, and in a few miles more into true marble, coloured by iron into a glowing orange or pale warm red—the peach-blossom marble, of which Verona is chiefly built—and then as you advance farther into the hills into variegated marbles very rich and grotesque in their veinings."

After dilating on the magnificent landscape viewed from the top of this promontory, embracing the blue plain of Lombardy and its cities, Mr. Ruskin said:—

"I do not think that there is any other rock in all the world from which the places and monuments of so complex and deep a fragment of the history of its ages can be visible as from this piece of crag with its blue and prickly weeds. For you have thus beneath you at once the birthplaces of Virgil and of Livy — the homes of Dante and Petrarch, and the source of the most sweet and pathetic inspiration to your own Shakspeare—the spot where the civilization of the Gothic kingdoms was founded on the throne of Theodoric; and there, whatever was strongest in the Italian race redeemed itself into life by its league against Barbarossa; the beginning of the revival of natural science and medicine in the schools of Padua; the centre of Italian chivalry, in the power of the Scaligers; of Italian cruelty, in that of Ezzelin; and, lastly, the birthplace of the highest art; for among those hills, or by this very Adige bank, were born Mantegna, Titian, Correggio, and Veronese."

Mr. Ruskin then referred to a series of drawings and photographs taken at Verona by himself and his assistants, Mr. Burgess and Mr. Bunney, which he had divided into three series, and of which he had furnished a number of printed catalogues illustrated with notes.

1. "Lombard, extending to the end of the twelfth century, being the expression of the introduction of Christianity into barbaric minds; Christianization.

2. "The Gothic period. Dante's time, from 1200 to 1400 (Dante beginning his poem exactly in the midst of it, in 1300); the period of vital Christianity, and of the development of the laws of chivalry and forms of imagination which are founded on Christianity.

3. "The first period of the revival, in which the arts of Greece and some of its religion return and join themselves to Christianity; not taking away its sincerity or earnestness, but making it poetical instead of practical. In the following period even this poetical Christianity expired; the arts became devoted to the pursuit of pleasure, and in that they persist except where they are saved by a healthy naturalism or domesticity.

1. "The Lombardic period is one of savage but noble life gradually subjected to law. It is the forming of men, not out of clay but wild beasts. And art of this period in all countries, including our own Norman especially, is, in the inner heart of it, the subjection of savage or terrible, or foolish and erring life, to a dominant law. It is government and conquest of fearful dreams. There is in it as yet no germ of true hope—only the conquest of evil, and the waking from darkness and terror. The literature of it is, as in Greece, far in advance of art, and is already full of the most tender and impassioned beauty, while the art is still grotesque and dreadful; but, however wild, it is supreme above all others by its expression of governing law, and here at Verona is the very centre and utmost reach of that expression.

"I know nothing in architecture at once so exquisite and so wild and so strange in the expression of self-conquest achieved almost in a dream. For observe, these barbaric races, educated in violence—chiefly in war and in hunting—cannot feel or see clearly as they are gradually civilized whether this element in which they have been brought up is evil or not. They *must* be good soldiers and hunters—that is their life; yet they know that killing is evil, and they do not expect to find wild beasts in heaven. They have been trained by pain, by violence, by hunger and cold. They know there is a good in these things as well as evil: they are perpetually hesitating between the one and the other thought of them. But one thing they see clearly, that killing and hunting, and every form of misery, pleasure, and of passion, must somehow at last be subdued by law, which shall bring good out of it all, and which they feel more and more constraining them every hour. Now, if with this sympathy you look at their dragon and wild beast decoration, you will find that it now tells you about these Lombards far more than they could know of themselves. . . . All the actions, and much more the arts, of men tell to others, not only what the worker does not know, but what he never can know of himself—which you can only recognize by being in an element more advanced and wider than his. . . . In deliberate symbolism, the question is always—not what a symbol meant first or meant elsewhere, but what it means now and means here. Now, this dragon symbol of the Lombard is used of course all over the world; it means good here, and evil there; sometimes means nothing; sometimes everything. You have always to ask what the man who here uses it means by it. Whatever is in his mind, that he is sure partly to express by it; nothing else than that can he express by it."

2. In the second period Mr. Ruskin said was to be found "the highest development of Italian character and chivalry, with an entirely believed Christian religion; you get, therefore, joy and courtesy, and hope, and a lovely peace in death. And with these you have two fearful elements of evil. You have first such confidence in the virtue of the creed that men hate and persecute all who do not

accept it. And worse still, you find such confidence in the power of the creed that men not only can do anything that is wrong, and be themselves for a word of faith pardoned, but are even sure that after the wrong is done God is sure to put it all right again for them, or even make things better than they were before. Now, I need not point out to you how the spirit of persecution, as well as of vain hope founded on creed only, is mingled in every line with the lovely moral teaching of the 'Divina Commedia'—nor need I point out to you how, between the persecution of other people's creeds and the absolution of one's own crimes, all Christian error is concluded."

In relation to this Mr. Ruskin referred to the history of the founder of the power of the Scalas, Mastino, a simple citizen, chosen first to be podestà and then captain of Verona, for his justice and sagacity, who, although wise and peaceful in his policy, employed the civil power in the persecution of heresy, burning above two hundred persons; and he also related how Can Signorio della Scala on his death-bed, after giving a pious charge to his children, ordered the murder of his brother—examples of the boundless possibility of self-deception. One of these children killed the other, and was himself driven from the throne: so ending the dynasty of the Scalas. Referring to his illustrations, Mr. Ruskin pointed out the expressions of hope, in the conquest of death, and the rewards of faith, apparent in the art of the time. The Lombard architecture expresses the triumph of law over passion, the Christian, that of hope over sorrow.

Mr. Ruskin concluded his remarks on this period by commenting on the history and the tomb of Can Grande della Scala, a good knight and true, as busy and bright a life as is found in the annals of chivalry.

3. "The period when classical literature and art were again known in Italy, and the painters and sculptors, who had been gaining steadily in power for two hundred years—power not of practice merely, but of race also—with every circumstance in their favour around them, received their finally perfect instruction, both in geometrical science, in that of materials, and in the anatomy and action of the human body. Also the people about them—the models of their work—had been perfected in personal beauty by a chivalric war; in imagination by a transcendental philosophy; in practical intellect by stern struggle for civic law; and in commerce, not in falsely made or vile or unclean things, but in lovely things, beautifully and honestly made. And now, therefore, you get out of all the world's long history since it was peopled by men till now—you get just fifty years of perfect work. Perfect. It is a strong word—it is also a *true* one. The doing of these fifty years is unaccusably Right, as art; what its sentiment may be—whether too great or too little—whether superficial or sincere—is another question, but as artists' work it admits no conception of anything better.

"It is true that in the following age, founded on the absolutely stern rectitude of this, there came a phase of gigantic power and of exquisite ease and felicity which possess an awe and a charm of their own. They are more inimitable than the work of the perfect school. But they are not *perfect*." . . . .

This period Mr. Ruskin named "the 'Time of the Masters,' Fifty Years, including Luini, Leonardo, John Bellini, Vettor Carpaccio, Andrea Mantegna, Andrea Verrocchio, Cima da Corregliano, Perugino, and in date, though only in his earlier life, belonging to the school, Raphael. . . . The great fifty years was the prime of life of three men. John Bellini, born 1430, died at 90, in 1516; Mantegna, born 1430, died at 76, in 1506; and Vettor Carpaccio, who died in 1522."

"The object of these masters is wholly different from that of the former school. The central Gothic men always want chiefly to impress you with the facts of their subject; but the masters of this finished time desire only to make everything dainty and delightful. We have not many pictures of the class in England, but several have been of late added to the National Gallery, and the Perugino there, especially the compartment with Raphael and Tobit, and the little St. Jerome by John Bellini, will perfectly show you this main character—pictorial perfectness and deliciousness—sought before everything else. You will find, if you look into that St. Jerome, that everything in it is exquisite, complete, and pure; there is not a particle of dust in the cupboards, nor a cloud in the air; the wooden shutters are dainty, the candlesticks are dainty, the saint's scarlet hat is dainty, and its violet tassel, and its ribbon, and his blue cloak and his spare pair of shoes, and his little brown partridge—it is all a perfect quintessence of innocent luxury—absolute delight, without one drawback in it, nor taint of the Devil anywhere." . . . .

After dilating on several other pictures of this class, giving evidence of the entire devotion of the artists of the period to their art and work, Mr. Ruskin adverted to the second part of his discourse, the rivers of Verona. "There is but one river at Verona, nevertheless Dante connects its name with that of the Po when he says of the whole of Lombardy,—

> 'In sul paese, ch' Adice e Po riga,
> Solea valore e cortesia trovarsi
> Prima che Federigo avesse briga.'

I want to speak for a minute or two about those great rivers; because in the efforts that are now being made to restore some of its commerce to Venice precisely the same questions are in course of debate which again and again, ever since Venice was a city, have put her senate at pause—namely, how to hold in check the continually advancing morass formed by the silt brought down by the Alpine rivers. Is it not strange that for at least six hundred years the Venetians have been contending with those rivers at their *mouths*—that is to say,

where their strength has become wholly irresistible—and never once thought of contending with them at their sources, where their infinitely separated streamlets might be, and are meant by Heaven to be, ruled as easily as children? And observe how sternly, how constantly the place where they are to be governed is marked by the mischief done by their liberty. Consider what the advance of the delta of the Po in the Adriatic signifies among the Alps. The evil of the delta itself, however great, is as nothing in comparison of that which is in its origin.

"The gradual destruction of the harbourage of Venice, the endless cost of delaying it, the malaria of the whole coast down to Ravenna, nay, the raising of the bed of the Po, to the imperilling of all Lombardy, are but secondary evils. Every acre of that increasing delta means *the devastation of part of an Alpine valley, and the loss of so much fruitful soil and ministering rain.* Some of you now present must have passed last year through the valleys of the Toccia and Ticino. You know therefore the devastation that was caused there, as well as in the valley of the Rhone, by the great floods of 1868, and that ten years of labour, even if the peasantry had still the heart for labour, cannot redeem those districts into fertility. What you have there seen on a vast scale takes place to a certain extent during every summer thunderstorm, and from the ruin of some portion of fruitful land the dust descends to increase the marshes of the Po. But observe further—whether fed by sudden melting of snow or by storm—every destructive rise of the Italian rivers signifies the loss of so much power of irrigation on the south side of the Alps. You must all well know the look of their chain—seen from Milan or Turin late in summer—how little snow is left, except on Monte Rosa, how vast a territory of brown mountain-side heated and barren, without rocks, yet without forest. There is in that brown-purple zone, and along the flanks of every valley that divides it, another Lombardy of cultivable land; and every drift of rain that swells the mountain torrents if it were caught where it falls is literally rain of gold. We seek gold beneath the rocks; and we will not so much as make a trench along the hill-side to catch it where it falls from heaven, and where, if not so caught, it changes into a frantic monster, first ravaging hamlet, hill, and plain, then sinking along the shores of Venice into poisoned sleep. Think what that belt of the Alps might be—up to four thousand feet above the plain—if the system of terraced irrigation which even half-savage nations discovered and practised long ago in China and in Borneo, and by which our own engineers have subdued vast districts of farthest India, were but in part also practised here—here, in the oldest and proudest centre of European arts, where Leonardo da Vinci—master among masters—first discerned the laws of the coiling clouds and wandering streams, so that to this day his engineering remains unbettered by modern science: and yet in this centre of all human achievements of genius no thought has been

taken to receive with sacred art these great gifts of quiet snow and flying rain. Think, I repeat, what that south slope of the Alps might be; one paradise of lovely pasture and avenued forest of chestnut and blossomed trees, with cascades docile and innocent as infants, laughing all summer long from crag to crag and pool to pool, and the Adige and the Po, the Dora and the Ticino, no more defiled, no more alternating between fierce flood and venomous languor, but in calm clear currents bearing ships to every city and health to every field of all that azure plain of Lombard Italy. . . . .

"It has now become a most grave object with me to get some of the great pictures of the Italian schools into England; and that I think at this time—with good help—it might be contrived. Further, without in the least urging my plans impatiently on any one else, I know thoroughly that this which I have said *should* be done, *can* be done, for the Italian rivers, and that no method of employment of our idle able-bodied labourers would be in the end more remunerative, or in the beginnings of it more healthful and every way beneficial than, with the concurrence of the Italian and Swiss governments, setting them to redeem the valleys of the Ticino and the Rhone. And I pray you to think of this; for I tell you truly—you who care for Italy—that both her passions and her mountain streams are noble; but that her happiness depends not on the liberty, but the right government of both." [J. R.]

GENERAL MONTHLY MEETING,

Monday, February 7, 1870.

JOHN PETER GASSIOT, Esq. F.R.S. Vice-President, in the Chair.

Lord Brabazon.
Robert Dudley Baxter, Esq.
Miss Mary Browne.
Michael Carteighe, Esq.
Charles Lukey Collard, Esq.
Miss Anne Gassiot.
Mrs. J. H. Gladstone.
Rodolph Alexander Hankey, Esq.
Mark Henry Lackersteen, M.D. M.R.C.P. F.L.S. F.C.S.
George Andrew Spottiswoode, Esq.

were *elected* Members of the Royal Institution.

The special thanks of the Members were returned for the following addition to "the Donation Fund for the Promotion of Experimental Researches":—

W. D. Esq. (*5th donation*) £5 5 0

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Antiquaries, Society of—Archæologia, Vol. XLII. Part 1. 4to. 1869.
Proceedings, Vol. IV. Nos. 3–6. 8vo. 1868–9. 8vo.

Asiatic Society of Bengal—Journal, No. 156. 8vo.
Proceedings, 1869, Nos. 9, 10. 1869. No. 1. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. XX. Nos. 1, 2. 8vo. 1869–70.
Bavarian Academy of Science, Royal—Sitzungsberichte. 1869. I. Hefte 4. II. Hefte 1, 2, 3. 8vo.
Baxter, R. Dudley, Esq. M.R.I. (the Author)—English Parties and Conservatism. (K 97) 8vo. 1870.
British Museum Trustees—Catalogues of Dermaptera Saltatoria, Part 2. 1869. 8vo.
Guide Books. 16mo. 1869.
Hand-List of Birds. 8vo. 1869.
Chemical Society—Journal for Dec. 1869, Jan. 1870. 8vo.
Church, Professor A. H. M.A. M.R.I.—Practice with Science: a Series of Agricultural Papers, Vol. II. 8vo. 1869.
Delondre, M. Aug.—Produits Végétaux du Portugal. Par J. L. Soubeiran et A. Delondre. (L 15) 8vo. 1867.
Editors—Academy for Dec. 1869, Jan. 1870. 4to.
American Journal of Science for Nov. 1869. 8vo.
Artizan for Dec. 1869, Jan. 1870. 4to.
Athenæum for Dec. 1869, Jan. 1870. 4to.
Chemical News for Dec. 1869, Jan. 1870. 4to.
Engineer for Dec. 1869, Jan. 1870. fol.
Horological Journal for Dec. 1869, Jan. 1870. 8vo.
Journal of Gas-Lighting for Dec. 1869, Jan. 1870. 4to.
Mechanics' Magazine for Dec. 1869, Jan. 1870. 8vo.
Nature for Dec. 1869, Jan. 1870. 4to.
Pharmaceutical Journal for Dec. 1869, Jan. 1870. 8vo.
Photographic News for Dec. 1869, Jan. 1870. 4to.
Practical Mechanics' Journal for Dec. 1869, Jan. 1870. 4to.
Revue des Cours Scientifiques et Littéraires. Dec. 1869, Jan. 1870. 4to.
Scientific Opinion. Dec. 1869, Jan. 1870. 4to.
Gassiot, J. P. Esq. F.R.S. M.R.I. (the Author)—London and St. Katharine's Dock Company. 8vo. 1869.
Gyll, Gordon W. J. Esq. M.R.I. (the Translator)—The Voyage to Parnassus; Numantia, a Tragedy; and the Commerce of Algiers. By Cervantes. Translated from the Spanish. 16mo. 1870.
Harlem, Société Hollandaise des Sciences—Archives Néerlandaises des Sciences, Tome IV. 8vo. 1869.
Ironmongers' Company, London—John Nichol: Some Account of the Worshipful Company of Ironmongers. 4to. 1866.
Catalogue of the Antiquities and Works of Art exhibited at Ironmongers' Hall, May, 1861. Edited by G. R. French. 4to. 1869.
Linnean Society—Journal, Nos. 47, 52. 8vo. 1869–70.
Proceedings. 1869. 8vo.
London, Corporation of—Catalogue of Library, 9th Supplement. 8vo. 1869.
Manning, Frederick, Esq. M.R.I.—Rev. F. Leigh Colvile: The Worthies of Warwickshire. 1500–1800. 4to. 1869.
Mechanical Engineers' Institution, Birmingham—Proceedings, Aug. 1869, Part 2. 8vo.
Meteorological Society—Proceedings, No. 45. 8vo. 1870.
New Zealand Government—Statistics of New Zealand, 1868. Fol. 1869.
Norwegian University, Christiania, Royal—Thomas Saga Erkibyskups. Ved C. R. Unger. (Norwegian and Latin.) 8vo. 1869.
Le Glacier de Boium en Juillet, 1868. Par S. A. Sexe. 4to. 1869.
Norsk Meteorologisk Aarbog for 1868. 4to. 1869.
Photographic Society—Journal. Nos. 212, 213. 8vo. 1869–70.
Preussische Akademie der Wissenschaften—Monatsberichte, Sept. Oct. 8vo. 1869.
Roussillon, the Duke of (the Author)—Origines, Migrations: Philologie et Monuments Antiques. 8vo. 1867.
Royal Institute of British Architects—Sessional Papers, 1869–70. No. 3. 4to.
Royal Society of London—Proceedings, No. 115. 8vo. 1869.

Scottish Society of Arts, Royal—Transactions, Vol. XVIII. Part 1. 8vo. 1869.
Statistical Society—Journal, Vol. XXXII. Part 4. 8vo. 1869.
Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, Dec. 1869, Jan. 1870. 8vo.
Tyndall, Professor, LL.D. F.R.S. M.R.I. (the Author)—Faraday as a Discoverer. New edition. 16mo. 1870.
Victoria Institute—Journal, Vol. III. No. 13. 8vo. 1869.

WEEKLY EVENING MEETING,

Friday, February 11, 1870.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

WILLIAM B. CARPENTER, M.D. F.R.S.

On the Temperature and Animal Life of the Deep Sea.

THE present Discourse embodies the most important general results obtained by the Exploration of the Deep Sea in the neighbourhood of the British Isles, carried on during the summer of 1869 in H.M. Surveying vessel 'Porcupine,' with the view of completing and extending the inquiries commenced in the 'Lightning' expedition of 1868, of which an account was given by the speaker at the Friday evening meeting of April 9, 1869.*

The expedition of the 'Porcupine' was divided into three Cruises. The *first* of these, which was placed under the scientific charge of Mr. J. Gwyn Jeffreys, F.R.S., accompanied by Mr. William L. Carpenter as Chemical assistant, commenced from Galway near the end of May, and concluded at Belfast at the beginning of July. It was directed in the first instance to the South-west, then to the West, and finally to the North-west as far as the Rockall Bank. The greatest depth at which Temperature-sounding and Dredging were carried on in this cruise was 1476 fathoms; and these operations, through the excellent equipment of the 'Porcupine' and the skill of her commander, Capt. Calver, were so successfully performed, that it was confidently anticipated that still greater depths might be reached with an equally satisfactory result.

The *second* Cruise, which was under the scientific charge of Prof. Wyville Thomson, F.R.S., with Mr. Hunter as Chemical assistant, was consequently directed to the nearest point at which a depth of 2500 fathoms was known to exist, *viz.* the northern extremity of the Bay of Biscay, about 250 miles to the west of Ushant. In this cruise Temperature-sounding and Dredging were carried down to the extraordinary depth of 2345 fathoms, or *nearly three miles*—a depth nearly equal to the height of Mont Blanc, and exceeding by more than 500 fathoms that from which the Atlantic Cable was recovered. This sea-bed, on which the pressure of the superincumbent water is nearly

* Proceedings of the Royal Institution, vol. v., p. 503.

three tons for every square inch, was found to support an abundance of Animal life; about 1½ cwt. of "Atlantic mud," chiefly consisting of *Globigerinæ*, having been brought up in the dredge, together with various types of higher animals, Echinoderms, Annelids, Crustaceans, and Mollusks; among them a new Crinoid—referable, like the *Rhizocrinus*, whose discovery by M. Sars, jun., had been the starting-point of the present inquiry—to the *Apiocrinite* type which flourished during the Oolitic period.

The *third* Cruise was under the scientific charge of the speaker, with Mr. P. H. Carpenter as Chemical assistant; but he had the great advantage of being accompanied by his colleague Prof. Wyville Thomson, who, as in the 'Lightning' expedition, took the entire superintendence of the Dredging operations. The object of this cruise, which commenced in the middle of August and terminated in the middle of September, was a more thorough exploration of the area between the North of Scotland and the Faroe Islands, which had been found in the 'Lightning' expedition to afford results of peculiar interest in regard alike to the inequality of Temperature and to the distribution of Animal life on the sea-bed, which here ranges between the comparatively shallow depths of from 350 to 650 fathoms—the last-named being the greatest depth to which dredging had been carried in 1868.

The weather, during nearly the whole of the 'Porcupine' expedition, was as favourable to its work, as during the greater part of the 'Lightning' expedition it had been unfavourable; and the results obtained not only far exceeded the most sanguine expectations of those who had promoted it, but may be said, without exaggeration, to be such as no previous Scientific exploration of so limited an extent and duration is known to have yielded.

The results of the *Temperature-soundings* will be first stated, with their bearing on the doctrines advanced in the former Discourse as probable inferences from the observations made during the 'Lightning' expedition. These observations indicated that two very different Submarine Climates exist in the deep Channel which lies E.N.E. and W.S.W. between the North of Scotland and the Faroe Banks; a *minimum* temperature of 32° having been registered in some parts of this channel, whilst in other parts of it, at *the like depths*, and with *the same surface temperature* (never varying much from 52°), the *minimum* temperature registered was never lower than 46°,—thus showing a difference of 14°. It could not be positively asserted that these *minima* are the *bottom*-temperatures of the Areas in which they respectively occur; but it was argued that they must almost necessarily be so:—*first*, because it is highly improbable that Sea-water at 32° should overlie water at any higher temperature, which is specifically lighter than itself, unless the two strata have a motion in opposite directions sufficiently rapid to be recognizable; and *second*, because the nature of the Animal life found on the bottom of the Cold area, which consists of quartzose

sand including Volcanic particles, exhibited a marked correspondence with its presumed reduction of temperature, whilst the sea-bed of the Warm area is essentially composed of *Globigerina*-mud, and the Animal life which it supports is characteristic of the warmer-temperate seas.

This conclusion, it is obvious, would not be invalidated by any error arising from the effect of Pressure on the bulbs of the Thermometers; since, although the *actual minima* might be, as was then surmised, from 2° to 4° below the *recorded minima*, the *difference* between temperatures taken at the same or nearly the same depths would remain unaffected.

The existence in the Cold area of a *minimum* temperature of 32°, with a Fauna essentially Boreal, could not, it was argued, be accounted for in any other way than by the supposition of an under-current of Polar water coming down from the North or North-east: whilst, conversely, the existence in the Warm area of a *minimum* temperature of 46°, extending to 500 or 600 fathoms' depth, in the latitude of 60° (being at least 8° above its isotherm), together with the warmer-temperate character of its Fauna, seemed equally indicative of a flow of Equatorial waters from the South or South-west.

It was further urged that if the existence of two such different Submarine Climates in close proximity can only be accounted for on the hypothesis of an Arctic stream and an Equatorial stream running side by side (the latter also spreading over the former in consequence of its lower specific gravity), these streams are to be regarded (like the Gulf Stream) as particular cases of a *great general Oceanic Circulation*, which is continually bringing the water cooled-down in the Polar regions into the deepest parts of the Equatorial ocean-basins, whilst the water heated in the Equatorial regions moves towards the Poles on or near the surface. Such a Circulation was long since pointed out to be as much a Physical necessity, as that interchange of *Air* between the Equatorial and Polar regions which has so large a share in the production of winds: but whilst Physical Geographers remained under the dominant idea that the Temperature of the Deep Sea is everywhere 39°, they could not fully recognize its importance.

These doctrines have been fully tested by the very numerous and careful Temperature-soundings taken in the 'Porcupine' expedition; and the result has been not merely to confirm them in every particular —so that they may now take rank as established facts,—but also to show that a Temperature 2½° *below the freezing-point of fresh water* may prevail over the sea-bed in a region far removed from the Polar, and that even this extreme reduction is by no means antagonistic to the existence of Animal life in great variety and abundance.

All the Temperature-soundings of the 'Porcupine' expedition were taken with Thermometers protected from the effects of pressure by the enclosure of the bulb of each instrument in an outer bulb, sealed round the neck of the tube; about three-fourths of the intervening space

being filled with spirit, but a small vacuity being left, by which any reduction in the capacity of the outer bulb is prevented from communicating pressure to the inner. This plan of construction, which was suggested by Professor W. A. Miller, has been so successfully carried into practice by Mr. Casella, that Thermometers thus protected have been subjected to a pressure of *three tons on the square inch*, in a testing-machine devised for the purpose, without undergoing more than a very slight elevation, of which a part (at least) is attributable to the heat given out by the compression of the water in which they were immersed: whilst the very best Thermometers of the ordinary construction were affected by the same pressure to the extent of 8° or 10°, the elevation in some instruments reaching as much as 50° or 60°.* Two of these protected Miller-Casella thermometers were used in each observation; and they always agreed within a fraction of a degree. The same pair was used throughout the Expedition; and notwithstanding that they were used for 166 separate observations, in which they travelled up and down nearly 100 miles, they came back in perfectly good order,—a result mainly due to the care with which they were handled by Captain Calver. It may be affirmed with great confidence that the Temperatures which they indicated were correct within 1° (Fahr.); an approximation quite near enough for the scientific requirements of the case.

In order to connect the work of the 'Porcupine' with that of the 'Lightning' expedition, it will be desirable to commence with the *third* Cruise of the former, in which a detailed survey was made of the area traversed in the preceding year by the latter. In this Cruise *bottom*-soundings were taken at thirty-six different stations, at depths varying from 100 to 767 fathoms; of these, seventeen were in the Cold area and fourteen in the Warm, whilst five exhibited intermediate temperatures, in accordance with their border position between the two. In order to ascertain whether the *minimum* Temperatures thus obtained were really the temperatures of the bottom, *Serial* soundings were taken at three stations, of which one was in the Warm area and two in the Cold,—the temperature at different depths between the surface and the bottom being ascertained by successive observations, at the same points, at intervals of 50 or 100 fathoms. All these results agreed extremely well with each other; and they closely accorded with the fifteen observations made in the 'Lightning' expedition, when the requisite correction for pressure (from 2° to 3°, according to the depth) was applied to the latter. (Table I., p. 81.)

* See Professor W. A. Miller's 'Note upon a Self-Registering Thermometer, adapted to Deep Sea Soundings,' in 'Proceedings of the Royal Society,' June 17, 1869.—The same principle had been previously applied in thermometers constructed under the direction of Admiral Fitzroy; the space between the two bulbs, however, being occupied with mercury instead of spirit. Owing, however, to some imperfection in their construction, their performance was not satisfactory, and they were found very liable to fracture.

The following General Summary of these results brings into marked contrast the conditions of the Warm and Cold areas, which occupy respectively the W.S.W. and E.N.E. portions of the Channel between the north of Scotland and the Faroe Islands, and lie side by side in its midst.

The *surface*-temperature may be said to be everywhere nearly the same, *viz.* 52°; the variations above or below this being attributable either to Atmospheric differences (as wind, sunshine, &c.) or to difference of Latitude. Alike in the Warm and the Cold areas there was a fall of from 3° to 4° in the first 50 fathoms, bringing down the temperature at that depth to 48°. A slow descent took place nearly at the same rate in both areas through the next 150 fathoms; the temperature in the Warm area at the depth of 200 fathoms being 47°, whilst in the Cold it was 45°·7. It is below this depth that the marked difference shows itself. For whilst in the Warm area there is a slow and pretty uniform descent in the next 400 fathoms, amounting to less than *four degrees* in the whole, there is in the Cold area a descent of *fifteen degrees* in the next 100 fathoms, bringing down the temperature at 300 fathoms to 30°·8. Even this is not the lowest; for the Serial soundings taken at depths intermediate between 300 and 640 fathoms (the latter being the greatest depth met with in the Cold area, midway between the Faroe and the Shetland Islands) showed a further progressive descent; the lowest bottom-temperature met with being 29°·6. Thus, while the temperature of the superficial stratum of the water occupying the Cold area clearly indicates its derivation from the same source as the general body of water occupying the Warm area, the temperature of the deeper stratum, which may have a thickness of more than *two thousand* feet, ranges from the freezing-point of fresh water to 2½° below it. Between the two is a *stratum of intermixture* of about 100 fathoms thickness, which marks the transition between the warm superficial layer and the body of frigid water which occupies the deeper part of the Channel.

The shortest distance within which these two contrasted Submarine Climates were observed at corresponding depths, was about 20 miles; but a much smaller distance was sufficient to produce it when the depth rapidly changed. Thus near the southern border of the deep channel, at a depth of 190 fathoms, the bottom-temperature was 48°·7; while *only six miles* off, where the depth had increased to 445 fathoms, the bottom-temperature was 30°·1. In the first case, the bottom evidently lay in the warm superficial stratum; whilst in the second it was overflowed by the deeper frigid stream.

It seems impossible to account for these phenomena on any other hypothesis than that of the *direct derivation of this frigid water from the Arctic basin.* And this agrees very well with other facts observed in the course of the exploration. Thus:—(1) The rapid descent of temperature marking the "stratum of intermixture" began about 50 fathoms nearer the surface in the most northerly portion of the Cold area examined, than it did in the most southerly, as might be expected

from the nearer proximity of the Cold stream to its source. (2) The sand covering the bottom contains particles of Volcanic minerals, probably brought down from Jan Mayen or Spitzbergen. (3) The Fauna of the Cold area has a decidedly Boreal type; many of the animals which abound in it having been hitherto found only on the shores of Greenland, Iceland, or Spitzbergen.

Although the Temperatures obtained in the Warm area do not afford the same striking evidence of the derivation of its whole body of water from a Southern source, yet a careful examination of its condition seems fully to justify such an inference. For the water at 400 fathoms in Lat. 59½° was only 2°·4 colder than water at the same depth at the northern border of the Bay of Biscay, in a Latitude more than 10° to the south, where the surface-temperature was 62°·7; and the approximation of the two temperatures is yet nearer at still greater depths, the bottom-temperature at 767 fathoms at the former station being 41°·4, whilst the temperature at 750 fathoms at the latter point was 42°·5. Now, as it may be certainly affirmed that the lowest temperature observed in the Warm area is considerably above the isotherm of its latitude, and that this elevation could not be maintained against the cooling influence of the Arctic stream but for a continual supply of heat from a warmer region, the inference seems inevitable that the bulk of the water in the Warm area must have come thither from the South-west. The influence of the Gulf Stream proper (meaning by this the body of superheated water which issues through the 'Narrows' from the Gulf of Mexico), if it reaches this locality at all—which is very doubtful—could only affect the *most superficial* stratum; and the same may be said of the surface-drift caused by the prevalence of south-westerly winds, to which some have attributed the phenomena usually accounted-for by the extension of the Gulf Stream to these regions. And the presence of the body of water which lies between 100 and 600 fathoms' depth, and the range of whose temperature is from 48° to 42°, can scarcely be accounted for on any other hypothesis than that of a *great general movement of Equatorial water towards the Polar area;* of which movement the Gulf Stream constitutes a peculiar case modified by local conditions. In like manner, the Arctic Stream which underlies the warm superficial stratum in our Cold area, constitutes a peculiar case, modified by the local conditions to be presently explained, of *a great general movement of Polar water towards the Equatorial area*, which depresses the temperature of the deepest parts of the great Oceanic basins nearly to the freezing-point.

During the *first* and *second* Cruises of the 'Porcupine,' the Temperature of the eastern border of the great North Atlantic basin was examined at various depths between from 54 to 2435 fathoms, and in widely different localities, ranging from Lat. 47° to Lat. 55°. The *bottom*-temperature was ascertained at thirty stations, and *serial* soundings were taken at seven stations; making the total number of observations eighty-four. (Table II., p. 82.) Amongst all these the coincidence

of temperatures at corresponding depths is extraordinarily close; the chief differences showing themselves in the temperature of the *surface* and of the stratum immediately beneath it. A decided *superheating* is observable in this superficial stratum, not extending to a depth of much more than 70 or 80 fathoms, and more considerable at the Southern than at the Northern stations. Whether this 'superheating' is entirely due to the direct influence of solar heat, or depends in any degree on an extension of the Gulf Stream as far as the southern part of the area examined, is a question which can only be resolved by the determination of its relative amount at different seasons. Between 100 and 500 fathoms, the rate of decrement is very slow, averaging only about 3° in the whole, or three-fourths of a degree for every 100 fathoms; and this body of water has a temperature so much above the isotherm of the northern stations at which the observations were made, as decidedly to indicate that it must have found its way thither from a Southern source. Between 500 and 750 fathoms, however, the rate of decrease becomes much more rapid, the reduction being 5°·4, or above 2° per 100 fathoms; while between 750 and 1000 fathoms it amounts to 3°·1, bringing down the temperature at the latter depth to an average of 38°·6. Beneath this there is still a slow progressive reduction with increase of depth, the temperature falling a little more than 2° between 1000 and 2435 fathoms; so that at the last-named depth, the greatest at which it was ascertained, it was 36°·5.—Thus it is obvious either that the vast body of water occupying the deeper half of the Atlantic basin has been itself derived from a colder region, or that its temperature has been reduced by the diffusion through it of frigid water from a Polar source. The latter supposition best accords with the *gradual* depression of temperature exhibited between 500 and 1000 fathoms, which corresponds with the "stratum of intermixture" of the Cold area.

The Temperature-soundings recently taken by Commander Chimmo, R.N., and Lieutenant Johnson, R.N., at various points in the North Atlantic basin, when the requisite corrections are applied for the influence of pressure on the bulbs of the unprotected Thermometers employed by them, give results which are remarkably accordant with our own; so that it may be stated with confidence that the temperature of the deeper parts of the North Atlantic sea-bed is but a very few degrees above the freezing-point.

Now a glance at the North Polar region, as laid down either on a globe, or any projection of which the Pole is the centre, shows that the Polar basin is so much shut in by the northern shores of the European, Asiatic, and American continents, that its only communication with the North Atlantic basin—besides the circuitous passages leading into Hudson's and Baffin's Bays—is the space which intervenes between the eastern coast of Greenland and the north-western portion of the Scandinavian peninsula. If, therefore, there be any such general interchange of Polar and Equatorial water as that for which we have argued, the Arctic current must flow through the deeper portions of

this interspace, at the north of which lies Spitzbergen, whilst Iceland and the Faroes lie in the middle of its southerly expanse. Now in the channel that lies between Greenland and Iceland, the depth is such as to give a free passage to such a frigid stream; but between Iceland and the Faroe Islands there is no depth so great as 300 fathoms at any part, except in a narrow channel at the south-east corner of Iceland; so that an effectual barrier is thus interposed to any movement of frigid water at a depth exceeding this. A similar barrier is presented, not merely by the plateau on which the British Islands rest, but also by the bed of the North Sea; the shallowness of which must give to such a movement a not less effectual check than would be afforded by an actual coast-line uniting the Shetland Islands and Norway. Consequently, it is obvious that a flow of ice-cold water, at a depth exceeding 300 fathoms from the surface, down the north-eastern portion of this interspace, *can* only find its way southwards through the deeper portion of the channel between the Faroe and Shetland Islands; which will turn it into a W.S.W. direction between the Faroe Islands and the north of Scotland, and finally discharge such part of it as has not been neutralized by the opposing stream coming up from the south-west, into the great North Atlantic basin, where it will meet the Icelandic and Greenland currents, and unite with them in diffusing frigid waters through its deeper portion. In thus spreading itself, however, the frigid water will necessarily mingle with the mass of warmer water with which it meets, and will thus have its own temperature raised, whilst lowering the general temperature of that mass; and hence it is that we do not find the temperature of even the greatest depths of the Atlantic basin nearly so low as that of the comparatively shallow channel which feeds it with Arctic water.

It may be questioned, however, whether the whole body of Arctic water that finds its way through the channels just indicated, could alone maintain so considerable a reduction in the temperature of the enormous mass which lies below 1000 fathoms in the Atlantic basin; subject as this must be to continual elevation by the surface-action of the sun on its southern portion. And as the few reliable observations on Deep-sea Temperatures under the Equator indicate that even there a temperature not much above 32° prevails, it seems probable that part of the cooling effect is due to the extension of a flow of frigid water from the Antarctic Pole, even north of the Tropic of Cancer. Of such an extension there is evidence in the Temperature-soundings recently taken in H.M.S. 'Hydra' between Aden and Bombay, where the cooling influence could scarcely have been derived from any other source than the Antarctic area.*

* The lowest temperature *actually observed* in these soundings was 36½°. The temperature of 33½° given in the previous Discourse, as existing below 1800 fathoms, proves to have been only an *estimate* formed by Captain Shortland under the idea that the rate of reduction observed at smaller depths would continue uniform to the bottom, which the Serial soundings of the 'Porcupine' prove to be by no means the case.

The unrestricted communication which exists between the Antarctic area and the great Southern Ocean-basins would involve, if the doctrine of a general Oceanic circulation be admitted, a much more considerable interchange of waters between the Antarctic and Equatorial areas, than is possible in the Northern hemisphere. And of such a free interchange there seems adequate evidence; for it is well known to navigators that there is a perceptible 'set' of warm surface-water in all the Southern oceans towards the Antarctic Pole; this 'set' being so decided in one part of the Southern Indian Ocean, as to be compared by Capt. Maury to the Gulf Stream of the North Atlantic.* Conversely, it would appear from the application of the necessary pressure-correction to the temperatures taken in Sir James Ross's Antarctic expedition, the voyage of the 'Venus,' &c., at depths greater than 1000 fathoms, that the bottom-temperature of the deepest parts of the Southern Oceanic basin really approaches the freezing-point, or is even below it. And if the temperature of the deeper portion of the *North* Pacific Ocean should be found to exhibit a depression at all corresponding to that of the North Atlantic, it must be attributed entirely to the extension of this Antarctic flow; since the depth of Behring's Strait, as well as its breadth, is so small as to permit no body of Arctic water to issue through that channel.

If further observations should substantiate the *general diffusion of a Temperature not much above the freezing-point over the deepest portions of the Ocean-bed, even in Intertropical regions*, as a result of a general deep movement of Polar waters towards the Equator, forming the complement of the *surface*-movement of Equatorial water towards the Poles, it is obvious that such diffusion must exert a very important influence on the distribution of Animal life; and, in particular, that we may expect to meet with forms which have hitherto been reputed essentially Arctic, in the deep seas of even the Intertropical region, and again in the shallower waters of the Antarctic area. Such, there is strong reason to believe, will prove to be the case. In his recent annual Address as President of the Royal Society, Sir Edward Sabine cites observations on this point made by Sir James Ross in his Antarctic expedition, as confirmatory of the view entertained by that distinguished navigator, "that water of similar temperature to that of the Arctic and Antarctic seas exists in the depths of the intermediate ocean, and may have formed a channel for the dissemination of species." The "similar temperature" believed by Sir James Ross to have had this general prevalence, seems to have been 39°; whereas the observations made in the 'Porcupine' expedition distinctly prove that a temperature even below 30° may be conveyed by Polar streams far into the Temperate zone, and that the general temperature of the deepest part of the North Atlantic sea-bed has more of a Polar character than he supposed.

Again, the Deep-sea Dredgings of the 'Porcupine' expedition have

* 'Physical Geography of the Sea,' §§ 748–750.

shown that many species of Mollusks and Crustacea previously supposed to be purely Arctic, range southwards in deep water as far as those dredgings extended—namely, to the northern extremity of the Bay of Biscay; and it becomes a question of high interest whether an extension of the same mode of exploration would not bring them up from the abysses of even Intertropical seas.

Now, as there must have been deep seas at all Geological epochs, and as the Physical forces which maintain the Oceanic circulation must have been in operation throughout, though modified in their local action by the particular distribution of land and water at each period, it is obvious that the presence of Arctic types of animal life in any Marine formation cannot be accepted as furnishing evidence *per se* of the general extension of Glacial action into temperate or tropical regions.—How far the doctrines now current on this point may need to be modified by the new facts now brought to bear on them, it will be for Geologists to determine; the question may be left in their hands with full assurance of a candid reception of the fresh evidence now adduced.

The general results of the Dredging operations carried on during the 'Porcupine' expedition will now be concisely stated.

In the first place they show conclusively that there is *no limit to the depth at which Animal life may exist on the Ocean-bed;* and that the types found at even the greatest depths may be *not less elevated in character* than those inhabiting shallower waters. It would even be premature yet to affirm that the higher types occur in less abundance and variety than at more moderate depths; for it is by no means impossible that the use of the improved method of collection devised by Capt. Calver,* which was employed with extraordinary success in the *third* Cruise, may make as large an addition to our knowledge of the Life of the sea-bottom explored by the Dredge in the *first* and *second* Cruises of the 'Porcupine,' as it has done in the case of the Cold area, where it revealed the astonishing richness of the bottom, which the 'Lightning' dredgings of the previous year had led us to regard as comparatively barren.

Secondly, they confirm our previous conclusion that *Temperature* exerts a much greater influence than Pressure on the distribution of Animal life. Not only have we found the same forms presenting themselves through an enormous *vertical* range—no amount of fluid Pressure being incompatible with their existence—but we have also, by a more complete survey of the relations of the Warm and Cold areas, established the very marked difference between the Faunæ of two contiguous portions of the sea-bed lying at the same depth, which was indicated by the 'Lightning' dredgings. It is remarkable, however, that

* This consists in the attachment of "hempen tangles" to the dredging apparatus, by which the floor of the ocean is *swept* as well as *scraped*. These tangles often came up loaded, when the dredge was nearly empty.

this difference showed itself more in the *Crustaceans*, *Echinoderms*, *Sponges*, and *Foraminifera*, than it did in the *Mollusca*, of which a considerable proportion were common to both Areas. The abundance and variety of Animal life on a bottom of which the Temperature is at least 2° (Fahr.) below the freezing-point of fresh-water, is a fact which has all the interest of surprise; and it is scarcely less remarkable that the forms of *Mollusks*, *Echinoderms*, and *Sponges*, which seem to be the characteristic inhabitants of this Cold area, should attain a very considerable size. The precise limitation of the *Globigerina*-mud and of the *Vitreous Sponges* to the Warm area, was a very striking manifestation of the influence of Temperature, and has very important Geological bearings.

Thirdly, they have largely added to the number of cases in which types that had been regarded as characteristic of earlier Geological periods, and to have long since become extinct, prove to be still existing in the depths of the Ocean; and greatly increase the probability that an extension of the like method of research to more distant localities would produce even more remarkable revelations of this character.

The doctrine propounded by Prof. Wyville Thomson, in the Report of the 'Lightning' expedition, as to the absolute continuity of the Cretaceous formation with the deposit of *Globigerina*-mud at present in progress on the North Atlantic sea-bed, has received such striking confirmation from the discovery of the persistence of numerous Cretaceous types, not merely in our own explorations, but also in those carried on by the United States Coast Survey in the Gulf of Mexico, that it may be fairly affirmed that the *onus probandi* rests upon those who assert that the formation of true Chalk has ever been interrupted since the Cretaceous period. That period is usually considered to have terminated with the elevation of the Cretaceous deposits of the European area into dry land. But according to the accepted doctrines of Geology, it is highly probable that, coincidently with the elevation of the European area, there was a gradual subsidence of what is now the Atlantic Sea-bed; so that the *Globigerinæ* of the former area, with many accompanying types of Animal life, would progressively spread themselves over the latter, as its conditions became favourable to their existence. And there seems no reason why they should not have maintained themselves in its deepest parts, through the comparatively small changes of level which took place in this portion of the earth's crust during the Tertiary epoch.

Fourthly, the 'Porcupine' explorations have enormously extended our knowledge of the British Marine Fauna; alike by the discovery of new types, and by the addition of types previously known only as inhabitants of other localities.—The *Mollusca* alone have as yet been fully examined; and Mr. J. Gwyn Jeffreys, whose authority upon this part of the subject is not second to that of any other Naturalist, reports as follows:—The total number of *species* of Marine Mollusca enumerated in his recently completed 'British Conchology' (exclud-

ing the Nudibranchs) is 451; and to these the 'Porcupine' expedition has added no fewer than 117, or more than *one-fourth*. Of these as many as fifty-six are undescribed, whilst seven were supposed to be extinct as Tertiary Fossils. Sixteen *genera*, including five which are undescribed, are new to the British seas. "All that I could do," he says, "by continual dredgings in comparatively shallow water during the last sixteen years was to add about eighty species to the number described by Forbes and Hanley. I regard the present (although a large) addition as merely an earnest of future discoveries. In fact the treasury of the deep is inexhaustible." The complete examination of the *Crustacea*, which are in the hands of the Rev. A. M. Norman, and of the *Annelids* which have been undertaken by M. Claparède and Dr. Macintosh, will probably yield results scarcely less striking. It is, however, in the *Echinoderms* and *Sponges*, which are being examined by Prof. Wyville Thomson; in the Stony Corals, which have been referred to Dr. P. M. Duncan; and in the *Foraminifera*, which constitute the speaker's own speciality, that the most interesting novelties present themselves.

An enormous addition has been made to the list of British *Echinodermata* by the discovery in our own seas of a number of species which had been previously known only as Norwegian or Arctic; and these often occurred in extraordinary abundance. One of the most interesting of these was the large and beautiful Feather-star, the *Antedon* (*Comatula*) *Eschrichtii*, hitherto known only as inhabiting the shores of Greenland and Iceland, but now found over all parts of our Cold area. On the other hand, the influence of Temperature was marked not only by the absence of many of the characteristically Southern types of this group, but by the *dwarfing* of others to such an extent that the dwarfed specimens might be regarded as specifically distinct, if it were not for their precise conformity in structure to those of the ordinary type. Thus the *Solaster papposa* was reduced from a diameter of *six* inches to *two*, and had never more than ten rays, instead of from twelve to fifteen; and *Asterocanthion violaceus* and *Cribella oculata* were reduced in like proportion. But, in addition, several Echinoderms have been obtained which are altogether new to science, most of them of very considerable interest. The discovery, at the depth of 2435 fathoms, of a living *Crinoid* of the Apiocrinite type, closely allied to the little *Rhizocrinus* (the discovery of which by the Norwegian naturalists was the starting-point of our own deep-sea explorations), but generically differing from it, cannot but be accounted a phenomenon of the greatest interest alike to the Zoologist and the Palæontologist. Another remarkable representative of a type supposed to have become extinct, occurred at depths of 440 and 550 fathoms in the Warm area; being a large *Echinidan* of the *Diadema* kind, the "test" of which is composed of plates separated from one another by membrane, instead of being connected by suture, so as to resemble an armour of flexible chain-mail, instead of the inflexible cuirass with which the ordinary *Echinida* are invested. This type bears a strong

resemblance to the very singular fossil from the White Chalk, described by the late Dr. S. P. Woodward, under the name of *Echinothuria floris*. Specimens were also obtained, both in the first and third Cruises, of a most interesting *Clypeastroid*, which is closely allied to the *Infulaster*—specially characteristic of the later Chalk.* These constitute only a sample of the interesting novelties belonging to this group, which our explorations brought to light.

Besides further additions to the remarkable group of *Vitreous Sponges*, which were made in the area over which the Globigerina-mud extends, a peculiar and novel form of *Sponge* was found to be one of the most generally diffused inhabitants of the Cold area. This sponge is distinguished by the possession of a firm branching axis, of a pale sea-green colour, rising from a spreading root, and extending itself like a shrub or a large branching *Gorgonia*. The axis is loaded with Siliceous spicules; and spicules of the same form are contained in the soft flesh which clothes it.

The *Foraminifera* collected in the 'Porcupine' expedition present features of no less interest, though their scale is so much smaller. The enormous mass of Globigerina-mud (sometimes almost pure, sometimes mixed with Sand) that everywhere covers the deep-sea bottom in the region explored, save where its temperature is reduced nearly to the freezing-point, may be judged of from the fact that in one instance the dredge brought up *half a ton* of it from a depth of 767 fathoms. The resemblance of this deposit to Chalk is greatly strengthened by the recognition of several characteristically Cretaceous types among the Foraminifera scattered through the mass of *Globigerinæ* of which it is principally composed; as also of the *Xanthidia*, frequently preserved in Flints. Not many absolute novelties presented themselves among the Foraminifera that form true Calcareous *shells;* the chief point of interest being the occurrence of certain types of high organization at great depths, and their attainment of a size that is only paralleled in much warmer latitudes, or in the Tertiary or yet older formations. This is especially the case with the *Cristellarian* group, which has a long Geological range; and also with the *Milioline*, of which specimens of unprecedented size presented themselves. The most interesting novelty was a beautiful *Orbitolite*, which, when complete, must have had the diameter of a sixpence, but which, from its extreme tenuity, always broke in the process of collection.—Of Arenaceous Foraminifera, however, which construct *tests* by cementing together Sand-grains, instead of producing shells, the number of new types is such as seriously to task our power of inventing appropriate generic names. Many of these types have a remarkable resemblance

* This was believed at the time to be an entirely new discovery; but since the return of the 'Porcupine' we have learned that a type generically, if not specifically, the same, had been obtained by Count Pourtales during his most recent dredgings in the Gulf of Mexico, and had been described by Mr. Alex. Agassiz under the name *Pourtalesia miranda*.

to forms previously known in the Chalk, the nature of which had not been recognized. Some of them throw an important light on the structure of two gigantic Arenaceous types from the Upper Green-sand, recently described by the speaker and Mr. H. B. Brady, an account of which will appear in the forthcoming part of the 'Philosophical Transactions;' and there is one which can be certainly identified with a form lately discovered by Mr. H. B. Brady in a clay-bed of the Carboniferous Limestone.

The question now arises, whether—as there must have been deep seas in all Geological periods, and as the changes which modified the climate and depth of the sea-bottom were for the most part very gradual—we may not carry back the continuity of the accumulation of *Globigerina*-mud on some part or other of the ocean-bed into Geological epochs still more remote; and whether it has not had the same large share in the production of the earlier Calcareous deposits, that it has undoubtedly had in that of the later. The Foraminiferal origin of certain beds of the Carboniferous Limestone, for example, appears to be indicated by the presence of *Globigerinæ*, long since observed by Professor Phillips in sections of them, as well as by the fact just stated. The sub-crystalline character of these rocks cannot be regarded as in any way antagonistic to such an idea of their origin, since it is perfectly well known that all traces of the organic origin of Calcareous rocks may be completely removed by subsequent metamorphism,—as in the Chalk of the Antrim coast.

What is the *source of Nutriment* for the vast mass of Animal life covering the abyssal Sea-bed, is a question of the greatest Biological interest. That Animals have no power of themselves generating the Organic Compounds which serve as the materials of their bodies—and that the production of these materials from the carbonic acid, water, and ammonia of the Inorganic world, under the influence of Light, is the special attribute of Vegetation—is a doctrine so generally accepted, that to call it in question would be esteemed a Physiological heresy. There is no difficulty in accounting for the alimentation of the higher Animal types, with such an unlimited supply of food as is afforded by the *Globigerinæ* and the *Sponges* in the midst of which they live, and on which many of them are known to feed. Given the *Protozoa*, everything else is explicable. But the question returns,—On what do these *Protozoa* live?

The hypothesis has been advanced that the food of the abyssal *Protozoa* is derived from *Diatoms* and other forms of minute Plants, which, ordinarily living at or near the surface, may, by subsiding to the depths, carry down to the animals of the sea-bed the supplies they require. Our examination of the surface-waters, however, has afforded no evidence of the existence of such Microphytic vegetation in quantity at all sufficient to supply the vast demand; and the most careful search in the Globigerina-mud has failed to bring to light more than a very small number of specimens of these siliceous envelopes of

Diatoms, which would most assuredly have revealed themselves in abundance, had these Protophytes served as a principal component of the food of the *Protozoa* that have their dwelling-place on the sea-bed.—Another hypothesis has been suggested, that these *Protozoa*, which are so near the border of the Vegetable kingdom, may be able, like Plants, to generate Organic Compounds for themselves—manufacturing their own food, so to speak, from Inorganic materials. But it is scarcely conceivable that they could do this without the agency of Light; and, as it is obviously the want of that agency which excludes the possibility of Vegetation in the abysses of the ocean, the same deficiency would prevent Animals from carrying on the like process.

A possible solution of this difficulty, offered by Professor Wyville Thomson in a Lecture delivered last spring, has received so remarkable a confirmation from the researches made in the 'Porcupine' expedition, that it may now be put forth with considerable confidence. It is, he remarked, the distinctive character of the *Protozoa*, that "they have no special organs of nutrition, but that they absorb water through the whole surface of their jelly-like bodies. Most of these animals secrete exquisitely-formed skeletons, sometimes of Lime, sometimes of Silica. There is no doubt that they extract both of these substances from the Sea-water, although Silica often exists there in quantity so small as to elude detection by chemical tests. All Sea-water contains a certain amount of Organic matter in solution. Its sources are obvious. All rivers contain a large quantity; every shore is surrounded by a fringe, which averages about a mile in width, of olive and red Sea-weeds; in the middle of the Atlantic there is a marine meadow, the Sargasso Sea, extending over 3,000,000 of square miles; the sea is full of Animals which are constantly dying and decaying; and the water of the Gulf Stream, especially, courses round coasts where the supply of organic matter is enormous. It is, therefore, quite intelligible that a world of animals should live in these dark abysses: but it is a necessary condition that they should chiefly belong to a class capable of being supported by absorption through the surface, of matter in solution; developing but little heat, and incurring a very small amount of waste by any manifestation of vital activity. According to this view, it seems highly probable that at all periods of the earth's history some form of the Protozoa—Rhizopods, Sponges, or both—predominated over all other forms of animal life in the depths of the Sea; whether spreading, compact, and reef-like, as in the Laurentian and Palæozoic *Eozöon;* or in the form of myriads of separate organisms, as in the *Globigerinæ* and *Ventriculites* of the Chalk."*

During each Cruise of the 'Porcupine,' samples of Sea-water obtained from various depths, as well as from the surface, at stations far removed from land, were submitted to the Permanganate test, after the method of Prof. W. A. Miller, with an addition suggested by Dr.

* "The Depths of the Sea," a Lecture delivered in the theatre of the Royal Dublin Society, April 10, 1869.

Angus Smith for the purpose of distinguishing the Organic matter *in a state of decomposition* from that which is only *decomposable;* with the result of showing the uniform presence of an appreciable quantity of matter of the latter kind, which, not having passed into a state of decomposition, may be *assimilable* as food by animals,—being, in fact, Protoplasm in a state of extreme dilution. And the careful analyses of larger quantities collected during the Third cruise, which have been since made by Dr. Frankland, have fully confirmed these results, by demonstrating the *highly azotized* character of this Organic matter, which presents itself in samples of Sea-water taken up at from 500 to 750 fathoms' depth, in such a proportion that its universal diffusion through the Oceanic waters may be safely predicated.

Until, therefore, any other more probable hypothesis shall have been proposed, the sustenance of Animal life on the ocean-bottom at any depth may be fairly accounted for on the supposition of Prof. Wyville Thomson, that the Protozoic portion of that Fauna is nourished by direct absorption from the dilute Protoplasm diffused through the whole mass of Oceanic waters, just as it draws from the same mass the mineral ingredients of the skeletons it forms. This diffused Protoplasm, however, must be continually undergoing decomposition, and must be as continually renewed; and the source of that renewal must lie in the *surface-life* of Plants and Animals, by which (as pointed out by Prof. Wyville Thomson) fresh supplies of Organic matter must be continually imparted to the Oceanic waters, being carried down even to their greatest depths by that *liquid diffusion* which was so admirably investigated by the late Professor Graham.

Not only, however, has the Nutrition of the Abyssal Fauna to be explained; its *Respiration* also has to be accounted for; and on this process also the results of the analyses of the Gases of the Sea-water made during the 'Porcupine' expedition throw very important light. Samples were collected not only at the surface, under a great variety of circumstances, but also from great depths; and the gases expelled by boiling were subjected to analysis according to the method of Prof. W. A. Miller—the adaptation of his apparatus to the exigencies of ship-board having been successfully accomplished during the First cruise by Mr. W. L. Carpenter. The general average of thirty analyses of surface-water gives the following as the percentage proportions:—25·1 Oxygen, 54·2 Nitrogen, 20·7 Carbonic Acid. This proportion, however, was subject to great variations, as will be presently shown. As a general rule, the proportion of Oxygen was found to diminish, and that of Carbonic Acid to increase, with the depth: the results of analyses of *intermediate* waters giving a percentage of 22·0 Oxygen, 52·8 Nitrogen, and 26·2 Carbonic Acid; whilst the results of analyses of *bottom*-waters gave 19·5 Oxygen, 52·6 Nitrogen, and 27·9 Carbonic Acid. But *bottom*-water at a comparatively small depth often contained as much Carbonic Acid and as little Oxygen as *intermediate* water at much greater depths; and the proportion of

Carbonic Acid to Oxygen in *bottom*-water was found to bear a much closer relation to the abundance of Animal life (especially of the more elevated types), as shown by the Dredge, than to its depth. This was very strikingly shown in an instance in which analyses were made of the gases contained in samples of water collected at every 50 fathoms, from 400 fathoms to the bottom at 862 fathoms, the percentage results being as follows:—

	750 fath.	800 fath.	Bottom, 862 fath.
Oxygen	18·8	17·8	17·2
Nitrogen	49·3	48·5	34·5
Carbonic Acid	31·9	33·7	48·3

The extraordinarily augmented percentage of Carbonic Acid in the stratum of water here immediately overlying the Sea-bed was accompanied by a great abundance of Animal life. On the other hand, the lowest percentage of Carbonic Acid found in bottom-water—*viz.* 7·9—was accompanied by a "very bad haul." In several cases in which the depths were nearly the same, the analyst ventured a prediction as to the abundance, or otherwise, of Animal life, from the proportion of Carbonic Acid in the bottom-water; and his prediction proved in every instance correct.

It would appear, therefore, that the increase in the proportion of Carbonic Acid, and the diminution in that of the Oxygen, in the abyssal waters of the Ocean, is due to the Respiratory process, which is no less a necessary condition of the existence of Animal life on the sea-bed, than is the presence of food-material for its sustenance. And it is further obvious that the continued consumption of Oxygen and liberation of Carbonic Acid would soon render the stratum of water immediately above the bottom completely irrespirable—in the absence of any antagonistic process of Vegetation—were it not for the upward diffusion of the Carbonic Acid through the intermediate waters *to the surface*, and the *downward* diffusion of Oxygen *from* the surface to the depths below. A continual interchange will take place *at* the surface between the gases of the Sea-water and those of the Atmosphere; and thus the Respiration of the Abyssal Fauna is provided for by a process of diffusion, which may have to operate through *three miles* or more of intervening water.

The varying proportions of Carbonic Acid and Oxygen in the *surface*-waters are doubtless to be accounted for in part by the differences in the amount and character of the Animal life existing beneath; but a comparison of the results of the analyses made during the agitation of the surface by wind, with those made in calm weather, showed so decided a reduction in the proportion of Carbonic Acid, with an increase in that of Oxygen, under the former condition, as almost unequivocally to indicate that superficial disturbance of the Sea by Atmospheric movement is absolutely necessary for its purification from the noxious effects of Animal decomposition. Of this view a

most unexpected and remarkable confirmation has been afforded by the following circumstance:—In one of the analyses of surface-water made during the Second cruise, the percentage of Carbonic Acid fell as low as 3·3, while that of Oxygen rose as high as 37·1; and in a like analysis made during the Third cruise, the percentage of Carbonic Acid was 5·6, while that of Oxygen was 45·3. As the results of every other analysis of surface-water were in marked contrast to these, it became a question whether they should not be thrown out as erroneous; until it was recollected that, whilst the samples of surface-water had been generally taken up from the *bow* of the vessel, they had been drawn in these two instances from *abaft the paddles*, and had thus been subjected to such a violent agitation in contact with the Atmosphere, as would pre-eminently favour their thorough aëration.

Hence, then, it may be affirmed that every disturbance of the Ocean-surface by Atmospheric movement, from the gentlest ripple to the most tremendous storm-wave, contributes, in proportion to its amount, to the maintenance of Animal life in its Abyssal depths—doing, in fact, for the aëration of the fluids of their inhabitants, just what is done by the heaving and falling of the walls of our own chest for the aëration of the blood which courses through our lungs. A perpetual calm would be as fatal to their continued existence, as the forcible stoppage of all Respiratory movement would be to our own. And thus universal stagnation would become universal death.

Thus it has been shown that the Bed of the Deep Sea, even in the immediate neighbourhood of our own shores, is an area of which the conditions have until lately been as completely unknown as those of the ice-bound regions of the Poles, or of the densest forests, the most arid deserts, the most inaccessible mountain-summits, that lie between the Tropics; and further, that by the systematic employment of the Sounding-apparatus, the Thermometer, and the Dredge, almost as complete a knowledge can be gained of those conditions, as if the explorer could himself visit the abyssal depths he desires to examine. Of the important discoveries in almost every department of Science, but more particularly in what Mr. Kingsley has well termed Bio-Geology, which may be anticipated from the continuation and extension of an inquiry of which the mere commencement has yielded such an abundant harvest, the speaker felt it scarcely possible to form too high an expectation. And, in conclusion, he referred to the systematic and energetic prosecution of Deep-sea explorations by the United States Coast Survey and by the Swedish Government—the results of which prove to be singularly accordant with those now briefly expounded,—as showing that other Maritime powers are strongly interested in the subject; and expressed the earnest hope that the liberal assistance of H.M. Government, which has already enabled British Naturalists to obtain the lead in this inquiry, would be so continued as to enable them to keep it in the future. In particular, he called attention to the suggestion lately thrown out by M. Alex. Agassiz, that an

arrangement might be made by our own Admiralty with the Naval authorities of the United States; by which a thorough survey, Physical and Biological, of the North Atlantic should be divided between the two countries; so that British and American explorers, prosecuting in a spirit of generous rivalry labours most important to the Science of the Future, might meet and shake hands on the Mid-Ocean.

[W. B. C.]

TABLE I.—TEMPERATURE of the SEA at DIFFERENT DEPTHS in the CHANNEL between the NORTH of SCOTLAND, the SHETLAND ISLES, and the FAROE ISLANDS; as ascertained by *Serial* and by *Bottom* SOUNDINGS. (N.B. The Roman Numerals indicate the 'Lightning' Temperature-Soundings, corrected for pressure.)

WARM AREA.						COLD AREA.						
Series 87.		Station No.	Depth.	Surface Temperature.	Bottom Temperature.	Series 64.		Ser. 52.	Station No.	Depth.	Surface Temperature.	Bottom Temperature.
Depth.	Temperature.					Depth.	Temperature.	Temperature.				
fathms.	°		fathms.	°	°	fathms.	°	°		fathms.	°	°
0	52·6					0	49·7	52·1				
50	48·1	73	84	52·7	48·8	50	45·5	48·5	70	66	53·4	45·2
		80	92	53·2	49·4				69	67	53·5	43·8
100	47·3					100	45·0	47·3	68	75	52·5	44·0
		71	103	53·0	48·6				61	114	50·4	45·0
		81	142	53·3	49·1				62	125	49·6	44·6
150	47·0	84	155	54·3	49·2	150	43·3	46·5	60	167	49·5	44·3
		85	190	53·9	48·7				IX.	170	52·0	41·0
200	46·8					200	39·6	45·6				
		74	203	52·5	47·7							
						250	34·3	38·4				
300	46·6					300	32·4	30·8				
									63	317	49·0	30·3
									65	345	52·0	29·9
									76	344	50·3	29·7
		50	355	52·6	46·2	350	31·4	..	54	363	52·5	31·4
		46	374	53·9	46·0	384	..	30·6				
400	46·1					400	31·0	..	86	445	53·6	30·1
		89	445	53·1	45·6	450	30·6	..				
		90	458	53·1	45·2				56	480	52·6	30·7
		49	475	53·6	45·4				53	490	52·1	30·0
500	45·1					500	30·1	..	X.	500	51·0	30·8
		VI.	530	52·5	44·8				58	540	51·5	30·8
		47	542	54·0	43·8				VIII.	550	53·0	29·8
		XV.	570	52·0	43·5	550	30·1	..	77	560	50·9	29·8
									59	580	52·7	29·7
600	43·0					600	29·9	..				
		XVII.	620	52·0	43·5				55	605	52·6	29·8
		XIV.	650	53·0	42·5				57	632	52·0	30·5
						640	29·6	..				
700												
		88	705	53·5	42·7							
767	41·4											

TABLE II.—TEMPERATURE of the SEA at DIFFERENT DEPTHS near the WESTERN MARGIN of the NORTH ATLANTIC BASIN, as ascertained by *Serial* and by *Bottom* SOUNDINGS.

SERIAL SOUNDINGS.								BOTTOM SOUNDINGS.			
Depth.	Temperature. Ser. 23.	Temperature. Ser. 42.	Temperature. Ser. 22.	Temperature. Ser. 19.	Temperature. Ser. 20.	Temperature. Ser. 21.	Temperature. Ser. 36.	Station No.	Depth.	Surface Temperature.	Bottom Temperature.
fathms.	°	°	°	°	°	°	°		fathms.	°	°
0	57·3	62·6	56·9	54·8	55·5	56·2	64·0				
50	..	53·2						27	54	55·6	48·3
								34	75	66·0	49·7
								6	90	54·0	50·0
								35	96	63·4	51·3
100	48·5	51·1						8	106	54·2	51·2
								24	109	57·7	46·5
150	..	50·9						7	159	53·2	50·4
								14	173	53·2	49·6
								18	183	53·2	49·4
200	48·0	50·5						13	208	53·6	49·6
250	..	50·2	48·5	48·0	48·5	48·3	50·5	4	251	53·5	49·5
300	47·8	49·6									
350	..	49·1						1	370	54·0	49·0
400	47·5	48·5									
450	..	47·6						15	422	52·2	47·0
								45	458	60·7	48·1
500	45·8	47·4	46·7	46·7	46·9	47·5	47·8				
								40	517	63·4	47·7
550	..	46·4									
600	44·5	45·5						41	584	63·4	46·5
630	43·4										
650	..	44·3									
700	..	43·6						12	670	52·2	42·6
								3	723	54·5	43·0
								36	725	63·9	43·9
750	..	42·5	42·0	41·2	41·6	42·4	41·3				
800	..	42·0									
								2	808	54·1	41·4
862	..	39·7						16	816	53·0	39·5
900								44	865	61·2	39·4
1000	..	..	38·8	38·5	38·8	38·5	38·3				
1200								43	1207	61·7	37·7
1263	..	..	37·3								
								28	1215	57·7	37·1
1250	..	..	..	..	37·7	37·9	37·7	17	1230	53·2	37·8
1300								29	1264	56·9	36·9
1360	..	..	..	37·4							
1400											
1443	..	..	..	..	37·0			32	1320	55·9	37·4
1450								30	1380	56·0	37·1
1476	..	..	..	..	..	36·9					
1500	..	..	..	..	..	..	37·2				
1750	..	..	..	..	..	..	36·7				
2090	..	..	..	..	..	..	36·3	37	2435	65·6	36·5

WEEKLY EVENING MEETING,

Friday, February 18, 1870.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

WILLIAM KINGDON CLIFFORD, Esq. B.A.

On Theories of the Physical Forces.

[Abstract deferred.]

WEEKLY EVENING MEETING,

Friday, February 25, 1870.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

CAPTAIN CHARLES W. WILSON, R.E.

On the Ordnance Survey of Sinai.

THE project of a systematic Survey of the Peninsula of Mount Sinai, with special reference to the Mosaic record of the Exodus, owes its origin to the Rev. Pierce Butler, late Rector of Ulcombe, Kent, who, unfortunately, was not spared to assist in carrying out a work for which he was so eminently qualified. After Mr. Butler's death it was felt by his friends and others whom he had interested in the scheme, that the best tribute to his memory would be the prosecution of the work which he had so much at heart, and for this purpose a fund was raised, of which Sir Roderick Murchison, Bart., Sir John Herschel, Bart., and Col. Sir Henry James, R.E., kindly consented to become Trustees. The sanction of the Right Honourable the Secretary of State for War was obtained, and the Survey was carried out under the direction of Col. Sir H. James, R.E., Director-General of the Ordnance Survey.

The Expedition consisted of Captains Wilson and Palmer, of the Royal Engineers; the Rev. F. W. Holland, who had already paid three visits to the Peninsula, and spent many months wandering over it on foot; Mr. E. H. Palmer, Fellow of St. John's College, Cambridge, a gentleman well known for his extensive knowledge of Eastern languages; Mr. Wyatt, who, at his own expense, accompanied the party as Naturalist; and four non-commissioned officers of the Royal Engineers from the Ordnance Survey,—one, Sergt.-Major McDonald, an experienced Photographer, who had previously been employed on the Ordnance Survey of Jerusalem.

The party were instructed to make special plans, on a scale of six inches to the mile, of Jebel Musa and Jebel Serbal, the two mountains which have been most often identified with Mount Sinai, and to make maps, on a scale of two inches to a mile, of as large a portion of the country as the funds would allow. Unfortunately they were only sufficient to cover the expense of surveying the western half of the Peninsula, embracing all possible routes by which the Israelites could have travelled on their march from Egypt to Sinai, but leaving the equally interesting question of the route which they took after leaving Mount Sinai unexamined.

The party were directed in addition to examine the numerous monastic and other remains in the Peninsula, to collect all possible information on the water supply, Meteorology, Geology, and Natural History, and Mr. Palmer was to pay special attention to the nomenclature of the country, the Bedawin traditions, and above all to the celebrated Sinaitic inscriptions and Egyptian monuments.

The Expedition was absent from England seven months, of which five were spent at work in the Peninsula with the following results.

Special surveys were made, on a scale of six inches to the mile, of Jebels Musa and Serbal, and the plans drawn.

In the execution of these surveys, which together embraced an area of 30½ square miles, the relative positions and altitudes of 68 trigonometrical stations were determined by triangulation from two base lines.

Some idea of the labour and difficulty of the work may be gathered from the fact that of the fifty-five stations actually observed from, a large number ranged from 1800 to 4800 feet above the camp of the surveying party, and that several of the peaks were very tough pieces of climbing, so much so that the instruments had in some cases to be pulled up over the ledges by a rope. Since our return to England two models have been constructed from the surveys and the rough models made on the ground, and these show more clearly than any description could do the different features of the rival mountains.

During the survey, on a smaller scale, of the western portion of the Peninsula, the direction of the true meridian was determined at six different places, and observations for time and latitude were made at thirty-six encampments, including eighty-three sets for time and 201 for latitude. Observations for longitude were also made at Jebel Musa. In this section the relative positions and altitudes of fifty-six mountain peaks, ranging to 8500 feet in height, were determined by triangulation, and referred to the level of the Red Sea by a series of Barometrical and Hypsometrical Observations. Seven hundred miles of route survey were made, laying down accurately the course of the principal valleys, and giving with the reconnaissance a survey of about 4000 square miles of country: a map of the entire Peninsula is now in course of preparation, which, in addition to the work of the Expedition, will show the latest discoveries of other travellers.

Before entering on the question of the Exodus and the route followed by the children of Israel, I propose giving a slight sketch of

the ruins in the Peninsula, the climate, the present resources of the Peninsula for supporting life, and the result of Mr. Palmer's labours amongst the Inscriptions and Bedawin.

Perhaps the most interesting remains are the stone houses and circles which are found in nearly every part of the Peninsula, and show that it must have been inhabited by a large settled population at a very early period. The houses are often found in clusters of from twenty to thirty, and in form are identical with those known as "bothan," or Beehive Houses, in Scotland. They are slightly elliptical in shape and from 40 to 50 feet in circumference. The walls rise perpendicularly for about 2 feet, after which the roof commences to close in and assume its peculiar "bee-hive" form, obtained by making each successive course of stone project slightly inwards beyond the one below it. The doors are 1′ 9″ wide and 1′ 8″ high. There is no trace of any tool having been used; and so primitive are the buildings, that no attempt has been made to round off the corners, which often project in an unpleasant manner. The houses are called by the Bedawin "nawamis," from a belief that they were built by the children of Israel to protect themselves from the mosquitos.

The stone circles are similar to those which in England and Scotland are called "Druids' circles," and are decidedly of a sepulchral character. In the centre of each circle is a cist about 4 feet long, 2 feet 5 inches broad, and 2 feet 6 inches deep, composed of four large stones, with their upper edges inclined slightly inwards, and covered by a heavy slab. Immediately round the cist is a circle of standing stones enclosing a cairn of small stones, and beyond this there is an outer circle, the stones of which are larger, and set in the ground so close that they touch each other.

The bodies were buried in the cists on their left sides, in that peculiar contracted or bent position which is usually considered one of the oldest forms of burial.

We were unfortunate in not finding any ornaments but a few marine shells and a shell bracelet; but Mr. Lord, who visited the country in the early summer of 1868, found "a small bracelet of copper associated with lance- and arrow-heads of flint, and a necklace of beads formed of spiral marine shells bored through for stringing." The arrow-head which Mr. Lord found is almost equal in finish to the best of those which are found in the ancient sepulchral chambers of England and Scotland. The stone circles range from 10 to 45 feet in diameter.

Besides the stone houses and circles there are a few places in which "cup-markings" are found, and foot or sole marks are cut on the face of the rock in several localities. One stone, with "cup markings," has a curious legend attached to it.

It is now perhaps impossible to discover who were the builders of these houses, &c., but it is suggested that they may possibly be the work of those Amalekites who fought against Israel in Rephidim.

The next remains which deserve attention are those of what may be called the monastic period, and these are far more numerous than has been generally supposed, fully bearing out what Burckhardt was the first to advance, that there were at one time as many as 700 or 800 monks resident in the Peninsula. The activity displayed by the old monks is quite surprising, and a great contrast to the indolence of the present inmates of St. Katherine's; wherever there is water, traces of cultivation and building are to be found, and in the vicinity of Musa and Feiran every available patch of ground on the mountains was formerly a garden.

The principal monastic establishments were at Musa, Feiran, and Tur.

At the former place a number of smaller monasteries, now in ruin, were built in the neighbourhood, probably in connection with Justinian's great Convent of the Transfiguration or St. Katherine, the only one which has been inhabited for many years.

The solid granite walls of the convent have been so much shaken by earthquakes and undermined by winter torrents that it was found necessary at the end of last century to support them by heavy buttresses, and even to rebuild the eastern face.

The ancient entrance to the convent, a fine old doorway which looks even older than the time of Justinian, is now closed, and all visitors have to enter by a postern. The old door was protected by a machiacoulis, on which there is an illegible Greek inscription of the same date as the erection of the convent. The interior of the convent, from the number and different ages of the buildings, is very puzzling; and there is a perfect labyrinth of passages turning and twisting in every direction, ascending and descending, exposed to the full glare of the sun or passing through dark tunnels. The church is a grand old building, and especially striking from the position in which it stands in the heart of the wilderness. The details of the capitals, &c., are highly interesting, as showing the early use of Christian symbols or emblems in church architecture, and the extensive use made of them in the reign of Justinian.

No two capitals are the same. One represents two sheep at the foot of the cross, from the arms of which hang an α and ω; above the cross is a vine-tendril with grapes, on either side a date palm, and beneath, as overcome by the cross, two snakes. Another has two sheep standing at an altar which supports a cross; a third, two birds at the cross; a fourth, two ibex butting at the cross; and others have the emblems of the Passion, shirt, nails, box with dice, &c., mixed with clusters of grapes, foliage, &c. The great Mosaic of the Transfiguration has been often described, but I am inclined to think that it is of later date than Justinian; at any rate, the medallions, of which good copies were brought home, bear no resemblance to the known portraits of Justinian and Theodora. Before leaving the convent it must be mentioned that on many of the arches and walls, notably on the face of the refectory, knightly pilgrims have been in the habit of cutting their

arms, and, in some cases, their names, and that some of these, dating back to the fourteenth century, are very interesting.

At Feiran there are the ruins of a convent and church on the small mound at the mouth of Wady Aleyat, a convent higher up the valley, and a series of tombs, cells, and chapels on Jebel Tahuneh, which have not hitherto received much attention. This mountain appears in old times to have been considered the sacred mountain of the place, and to have been of far more importance than Serbal itself; it is literally covered with tombs and chapels, and a well-constructed road, or rather series of steps, led to the summit which was crowned by a church, and from which there is the finest view of Serbal in the neighbourhood; the flight of steps, of which large portions remain, passed close to the numerous little chapels which appear to have been built over the cells of hermits, and may have been so many stations at which pilgrims offered their prayers. The church at the summit was at one period turned into a mosque.

The most remarkable feature connected with Feiran is the number of tombs which are found there. They are rectangular in shape, built of loose stones, and contain two or more interments according to their size. The tombs are built east and west, and the bodies are laid on their backs at full length with their feet to the east; they are without any doubt connected with the Monastery of Feiran, and it is curious to find that such different modes of burial were followed at places so near each other as Musa and Feiran; for at the former there is no trace of any stone tombs, all the interments appear to have been made in the ground, and the bones afterwards collected and placed in the crypt now in use. At the back of Serbal there are the remains of several monastic establishments in a valley (Wady Sigillyeh) which in scenery and perfect seclusion might rival the "Happy Valley of Rasselas;" shut in on each side by lofty precipices, it breaks through the mountains by an almost inaccessible gorge, and the approach to its head is by a tremendous descent over loose stones from the shoulder of Serbal. Nowhere is the energy of the monks seen more fully than in the wonderful road which they made over this pass, and the great staircase to the foot of the valley; and though now in ruin they are specimens of engineering skill which must strike every one with admiration.

The convents at Tur are not so interesting, but there are numbers of cells and chapels in the rocks along the sea coast which well repaid the time given to their investigation.

The climate of the mountain district of Sinai is perhaps the most agreeable in the world; the air is perfectly dry, and though the nights are cold in winter the day is always warm and pleasant; in summer some portions of the day are extremely hot, but the nights are always cool, and the Khamsin, or hot wind, which is so trying when met with on the plains, at that altitude, merely produces a feeling of great lassitude. The heat in summer on the plains, especially in the limestone districts, is very great; and when the Khamsin blows, the

mere act of existence becomes a labour, the air is filled with fine particles of sand, and there is a terrible feeling of depression and oppressiveness as if one were being weighed down to the ground and could scarcely breathe.

The rainfall appears to vary greatly in different years; the winter spent in the Peninsula by the Expedition was a remarkably dry one, whilst the preceding one had been very wet.

Snow falls every year in the higher mountains; but it never lies for any length of time, and rarely reaches lower down than 5500 feet above the sea. The country is subject to violent rain storms, which fill the dry beds of the valleys with roaring torrents, and are sometimes attended with loss of life. The storms are very partial, and the first symptom of a storm in the mountains may be a stream rushing down the valley. Mr. Holland was fortunate enough, though not without great personal risk to himself, to see one of these floods, or "seils," in the Wady Feiran.

The storm commenced at 4.30, and a few minutes after six the dry bed of the Wady, over 300 yards wide, was turned into a foaming torrent, 8 to 10 feet deep. Next morning a gently-flowing stream, a few yards wide, was all that remained; but the whole bed of the Wady was changed, nearly 1000 palm trees were swept away, and about thirty Bedawin were lost and buried in the *débris*. While at Tur we found traces of the flood from this storm that had come down Wady Sigillyeh, and which, after passing over 16 or 17 miles of dry desert, the plain of El Gaah, had a body of water between 4 and 5 feet deep and about 150 yards wide. Wellsted mentions a similar flood in 1832, which left an alluvial deposit one foot thick in the neighbourhood of Tur.

Small whirlwinds are very frequent, and rise up in the dry valleys as if by magic; on two occasions catching up instruments before they could be saved.

The colouring, especially at sunrise and sunset, is extremely brilliant, and in the evening there is often a most lovely atmospheric effect. About half an hour after the last trace of after-glow has died away, and when the stars are shining brightly, the whole sky to 45° above the horizon is suffused with a delicate blush pink, the true auroral flush, "celestial rosy red;" the effect of the dark mountain peaks against this background can hardly be described by words. Before leaving the subject of climate, it may be remarked as strange that no mention is made in the Bible of cold or frost in connection with the sojourn at Sinai, though the Israelites, coming from the low country of Egypt, must at first have suffered severely.

The present capabilities of the Peninsula for supporting life may be divided into three—water, food for man, and food for animals.

The water supply is far larger in the granitic mountain districts than has been generally supposed. In the neighbourhood of Jebel Musa there are at least three perennial streams, besides a large number of springs. In Wady Feiran there is the well-known stream, and at

Jebel Benat and Wady Sigillyeh there are streams which swell out into large pools in which one can swim about with comfort. The water is pure and limpid, very different from the springs in the limestone districts, which must always have been brackish. There is thus no difficulty in supposing a fair supply for the children of Israel even if the rainfall were not much greater than at present. In immediate connection with this is the question whether some of the lakes of which we have evidence in the alluviums of Wady Feiran, Wady Igne, and other places were in existence at the time of the Exodus, strong evidence has been brought to show that one at least existed where the Egyptian mines of Maghara were worked. There is not time to discuss this; but I may remark that the word "Horeb," which means a "place made dry by the running off of water," would have a strange significance if we supposed that the water of the lakes had run off not very long before Moses led the Israelites into the desert.

Of food for man there is little; but the resources might be largely increased, and during early Christian times there is no doubt that a considerable extent of ground was highly cultivated, and even now in the old gardens, which receive no attention, olive, plum, cherry, and other fruit trees flourish in the greatest luxuriance. There are also several large groves of palm trees, and we have reason to believe that the people who built the stone houses were agricultural, so that the country may have been well cultivated at the time of the Exodus. There is a fair supply of game, including ibex, hare, and four or five species of partridge; but of course very insufficient for a large host like that of the Israelites. The tamarisk, or tarfa, from which the manna distils, is generally distributed over the Peninsula, often in dense thickets; but it is impossible to believe that this, which is caused by insects during a few months of summer and is really a mild aperient medicine, can have any connection with the manna of the Bible; besides, the manna supply continued during their whole journey through country where the tamarisk is rarely if ever found.

The food for goats, sheep, oxen is now scant except after rain, when grass shoots up very rapidly and in comparative abundance. Whether the supply was formerly greater and more constant throughout the year depends on the rainfall, and this was probably greater than at present; at any rate, if we may judge from the figures of horses on the rocks, and the numerous Bedawi legends relating to them, there must have been within the present era vegetation sufficient for horses to have lived in the Peninsula.

Those who have visited Palestine, and know what it was, even as late as the Crusades, can understand how soon a Paradise may become a desert.

It is at first rather disappointing to find that few, if any, traces of the ancient Scriptural names remain in the Peninsula, even the name Sinai not being known in the native nomenclature. Still, if nothing has been discovered, Mr. Palmer has done good service in upsetting several theories which have been based on the apparent similarity of

names. This is specially the case with Jebal Sona and Jebel Serbal. In the first a trace of the word Sinai is supposed to have been found, but it is really the Mount of the Artisan. Serbal has been connected with Baal, and so considered to have been a sacred mountain, whereas it expresses the manner in which heavy rain 'shirts' or covers the summit.

It is the same with the Bedawi traditions, which have undoubtedly been influenced to a very great extent by monkish legend, so much so that it is not an easy matter to separate those which are of purely native origin. Some of these latter which Mr. Palmer has succeeded in bringing to light are highly interesting, especially one which places the rock from which Moses brought water, in Wady Feiran, not far below the position assigned by the earliest Christian tradition to Rephidim, and another which would seem to identify some peculiar remains with the camp of Kibroth Hattaavah.

The Bedawin of Sinai number about 4000 males. They are a quiet inoffensive race, and their poverty is such that their whole life is one long struggle for existence.

The Towara are not the aboriginal inhabitants of the Peninsula, but settled in it at the time of the Mohammedan Conquest. Their predecessors were a branch of the Aramæan race, of whom traces may possibly remain in the Jibaliyeh tribe, as names peculiar to them are found in the Aramæan (Sinaitic) inscriptions. The Jibaliyeh are looked down upon by the other Bedawin as not being of pure descent. They are supposed to owe their origin to the 400 Wallachian and other prisoners sent by Justinian for the service of the convent, and this is supported by their own tradition, a peculiar cast of countenance, and their names, indicating intermarriage with the Aramæans.

The Bedawin have frequently been termed irreligious by those who have not watched them closely; but this is far from being the case. It is true they do not go through the outward forms of Moslem devotion; but the following prayers, which every Bedawi offers up at sunrise and sunset, walking by his camel or lounging at home, will show that there is a strong religious feeling amongst them. Every Bedawi believes firmly that he wakes up with the power of Evil on one shoulder and the power of Good on the other, and that if he does not say his morning prayer the Evil One clings to him for the day.

Sunrise—Morning Prayer. "I seek refuge with the Great God from Satan accursed with stones. Deliver me from evil, provide for me, and for my brethren the faithful. O Lord! be gracious unto us, for our forefathers' sake. Lord, be gracious unto us; for a people that prospers is better than a people that strives. O Lord! uncover not our inmost faults; protect our children and our weaker friends. O Lord! provide for me, thou who providest for the blind hyæna!"

Sunset Prayer.—"O Lord! be gracious unto us. In all that we hear or see, in all that we say or do, be gracious unto us! Have mercy on our friends, who have passed away before us. I ask pardon

of the Great God; I ask pardon at the sunset, when every sinner turns to Him. Now and for ever I ask pardon of God. O Lord! cover us from our sins, guard our children, and protect our weaker friends."

Night Prayer.—"I lay down my head to rest, and the Lord is my security against remote evil and present harm."

With regard to the Sinaitic Inscriptions, Mr. Palmer determined on visiting the country to form his own opinion on the subject before criticizing the theories of others: for this purpose he devoted himself to copying and studying the inscriptions on the rocks, and after some time succeeded in deciphering them by a perfectly independent and self-interpreting process. A cast in plaster was taken of a bilingual tablet in Greek and Sinaitic, several photographs and paper impressions were taken of others, and Mr. Palmer and Mr. Holland brought home in addition copies of nearly 3000 inscriptions, which were taken under very trying circumstances. I may add that many of Mr. Palmer's conclusions are corroborated by the results obtained by the Count de Vogüé, and published in his work on the inscriptions in Central Syria. The inscriptions consist of detached sentences in an Aramæan dialect; for the most part proper names, with introductory formulæ; but as Mr. Palmer has not yet written his report, I am unable to give any translations.

The following extract relating to the character of the inscriptions is made from an entry in my journal after an examination of those in Wady Mukatteb:—

"The number of inscriptions in Wady Mukatteb may be estimated at about 2000. The height of the cliffs in the valley has been much exaggerated. The inscriptions are mostly on the lower hard bed of sandstone, or on blocks which have fallen from it; not a dozen occur on the upper beds. There is only one inscription which cannot easily be reached by climbing, and in this case the softer sandstone below has evidently been broken away since it was made; except in a few isolated cases inscriptions are never found more than a few yards from the road through the valley. In several places unmistakeable bilingual inscriptions, Sinaitic and Greek, by the same hand, are found. There are also many distinct Greek inscriptions of the same age as the Sinaitic, some of which contain Christian symbols, such as the ☧ and ✝; the latter, from the absence of Bible names, appear to be of early Christian date. The writers of the inscriptions possessed a very imperfect knowledge of Greek: names are frequently written backwards, as in the Sinaitic inscriptions, and letters such as P are often turned the wrong way. Egyptian names, as *Horus*, occur, but the prevailing names are those which end in Baal and Omru. The inscriptions are probably the work of a trading community settled in the Peninsula, and they are often accompanied by rude drawings of camels, horses, &c., and in one case, of a man leading a cow. Amongst the drawings are several of an obscene character, but the larger number are rude representations of the ibex or bedan, with enormously exagge-

rated horns; there are also figures of warriors, with sword and shield, or mounted on horseback with lances; and in one place there is a spirited representation of an ibex hunt, the hunter being on foot and accompanied by dogs. The inscriptions have been cut with flints, or roughly dotted on to the face of the rock with any hard stone."

The inscriptions are far more generally distributed over the Peninsula than has been supposed. There is a collection near Ain Hudhera almost as large as that in Wady Mukatteb, on the road from Feiran to Benat, and from Jebel Musa to Sherm there are numbers, and Mr. Palmer has lately found them at the head of Wady Gharundel and on the outskirts of the Tih. Inscriptions are also found in the valleys near Jebel Musa and Jebel Serbal. At the latter place they are found on three occasions under peculiar circumstances: 1st. On the monastic road to Sigillyeh in such positions that they must have been written after the road was made, and consequently after the introduction of Christianity; 2nd. In a cave near the summit of Serbal, where they are made with whitewash, evidently by the same men who built the signal tower, of which there are the remains close by; and 3rd. On Jebel Moneija, a hill overlooking Wady Aleyat: this is one of the places in which God is supposed to have spoken to Moses, and the Bedawin sacrifice there every year at the time of date harvest.

An examination was made of the Egyptian mines at Maghara and Sarabit el Khadem; a plan was made of the temple at the latter place, and photographs and paper impressions were taken of the tablets and inscriptions. These mines are well known, and have been described and figured by Lepsius, Laborde, Laval, and others; but Dr. Birch, the distinguished Egyptologist, who has kindly consented to contribute a description of them to the work which is now in course of preparation, has informed me that he is able to decipher many parts of the inscriptions from our photographs, which were illegible in the copies of previous travellers.

Several new inscriptions and tablets were found; one indicating that the miners were accompanied by their wives and families whilst working at Maghara. The oldest monument appears to be that of Cheops, the builder of the Great Pyramid, and from his reign down to a time posterior to the Exodus there is a pretty constant succession of tablets recording the working of the mines.

At Maghara may still be seen the ruins of the village in which the miners lived on an almost isolated hill opposite the mines, and connected with them by a causeway, of which there are still traces. The mines were worked for copper and turquoise, the latter possibly on account of its colouring matter, as no turquoises have yet been found in the ancient jewellery of the Egyptians.

A question has been raised as to whether the Mountain of the Law was really in what we call the Peninsula of Sinai: time will not permit me to enter upon this at present, but there seems to be no sufficient reason for believing that the present tradition, which is certainly as old as Josephus, is wrong, and I am able to announce that the

members of the Expedition have unanimously come to the conclusion that the mountain mass of Jebel Musa is the true Mount Sinai. Nowhere else in the Peninsula is there the same conjunction of mountain, plain, and water, and even when we come to minute details they agree in a most marked manner with the requirements of the Bible narrative.

In comparing the rival claims of Musa and Serbal, it will be as well to consider what are the topographical features which the Bible narrative appears to require in connection with Mount Sinai, and to examine how far each of them fulfils the requisite conditions.

They may be stated thus:—

1st. A mountain summit overlooking a place upon which the children of Israel could be assembled. It does not seem necessary to suppose that there must have been space in front of the Mount sufficient for their permanent encampment; indeed it would rather appear that the tents were scattered about in the neighbouring valleys, whence they could be easily summoned to take part in any solemn act, such as the delivery of the ten commandments.

At Musa there is the great cliff of the Ras Sufsafeh overlooking the plain of Er Rahah, which slopes gently down to its base, a remarkable geographical feature which would attract the attention of a traveller if there were no other circumstances connected with it.

At Serbal there is no such plain, but there is sufficient space for the host to have assembled in Wady Aleyat and Er Rimm in sight of the summit of Serbal, which, by the way, is invisible from Wady Ajeleh.

This arrangement would necessitate the assembly of the Israelites in two valleys separated from each other by a high ridge of granite, and the beds of the valleys are now and must always have been of the roughest description, and a direct contrast to the even plain of Er Rahah.

2nd. The place on which the Israelites assembled must have such a relation to the mountain, that the people could stand "at the nether part of the Mount" and yet "remove and stand afar off," and also that they could hear the voice of the Lord when he spake "out of the midst of the fire" and answered Moses "by a voice."

This condition is perfectly fulfilled at Musa, hardly at all at Serbal: in the former place the people could gather together at the foot of the Ras Sufsafeh, and retire up the slope of the Er Rahah; in the latter, those in Wady Er Rimm could not approach the foot of Serbal, and if they moved backwards would lose sight of the summit; which is moreover so situated that the sound of a voice from it would lose much of its force before reaching either Wady Aleyat or Wady Er Rimm.

3rd. The summit of the Mount of the Law should be a well-defined peak, visible from the nether part of the Mount as well as from afar off, and easily distinguished as the "top of the Mount" on which the Lord came down. This is eminently the case with the grand peak of the Ras Sufsafeh; but it would puzzle any one to select

any particular peak in the ridge of Serbal as that on which the Law was given.

There are in truth some ten or twelve peaks varying little in altitude, so little that from a distance the highest peak which stands a little back cannot be distinguished by the eye. The summit also cannot be seen from the foot of the mountain.

4th. The mountain should rise precipitously from the place of assembly: in Deut. iv. 11, the people are said to have stood "under it," and apparently at the same time to have been able to see the summit; it was also a mountain that could be touched. This again applies in a remarkable manner to the Cliff of Sufsafeh, not at all to Serbal, where, as we have seen, the real summit stands back from the face of the mountain.

5th. The Mount should be sufficiently isolated to allow of the possibility of setting bounds round it. The mass of Jebel Musa is isolated in a very peculiar manner from the mountains round by the two valleys, Wady Leja and Wady Ed Deir; so that there would be no difficulty in marking out a limit beyond which the people should not advance.

Serbal, on the contrary, is the culminating point of a great mountain mass, the whole of which would have to be enclosed by the bounds, as it would be almost impossible to isolate the summit itself.

6th. In Deut. ix. 21, it is said that the brook into which the dust of the golden calf was cast "descended out of the Mount," and it is evident, from many passages, that the supply of water at Sinai must have been ample.

Now at Serbal there is no such stream, but at Musa we find a stream to the present day coming down Wady Shreich, close to Ras Sufsafeh, and as it were out of the very heart of the mountain. It is curious to note that an earlier tradition than that of the present day made this Wady Shreich the route taken by Moses in ascending Sinai, and placed the scene of the worship of the golden calf on a hill near its mouth, close to the place at which the mould is now shown. The modern Aaron's mound was at that time the site of Moses' tent. In water supply, as mentioned before, the Musa district far surpasses that of Serbal, and of any other part of the Peninsula.

The arguments in favour of Serbal derived from its name and the occurrence of inscriptions there, have been already disposed of, and there only remain those derived from the accounts of Jerome and Eusebius, which are rather confused, and can, I think, be easily answered.

Another question still remains, that of whether the Ras Sufsafeh or the summit of Jebel Musa is the actual peak on which the Law was given. The actual summit of Musa is not visible from the plain of Er Rahah, and though the peak is precipitous towards the south and overlooks the Wady Sebaiyeh, the valley is at some distance from the mountain, is narrow, and the summit can only be seen from a small portion of it. It seems to me that this question is best met by supposing the delivery of the Law to have been from the summit of the

Ras Sufsafeh, and the instructions for the Tabernacle, &c., received by Moses during his forty days' retirement, to have been given on Jebel Musa, both peaks being really part of the same mountain. No one can help being struck by the seclusion of the Er Rahah plain in the inmost recesses of the mountains, and the grandeur of the surrounding scenery; indeed it might almost seem, like the remarkable place near Shechem where the Law was ratified, to have been specially made for the use which was afterwards made of it.

[C. W. W.]

WEEKLY EVENING MEETING,

Friday, March 4, 1870.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

E. J. Reed, Esq. C.B.

CHIEF CONSTRUCTOR OF THE NAVY,

On Iron-clad Ships.

Only eighty-three years ago the first iron vessel of which we have any record—a canal boat—was built; it is but fifty years since the first iron *ship*, which was also a *steamship*, was constructed; and but thirty years since iron sea-going ships came into extensive use. Only sixteen years ago the first armour-clad vessels—floating batteries—were built for service during the Crimean war; and it is but twelve years ago that the first iron-clad *ship*, 'La Gloire,' was commenced. Now, every ocean is traversed and every important port visited by iron vessels; the largest and finest merchant ships afloat are built of iron; a very large proportion of our mercantile marine is constructed of that material; and the three great naval powers—England, France, and America—possess together over a hundred and fifty iron-clad ships of all classes, while Russia, Prussia, Austria, Italy, and other powers probably have altogether as many more.

When the question of protecting ships against shot and shell first came to be discussed, there was naturally great diversity of opinion as to the best means to be employed. All classes of the public—including numerous clergymen—put forward schemes for the purpose, many of which were extremely singular. Iron armour was mostly in favour, but the methods proposed for its application were very various. "Arched," "angulated," "corrugated," "fluted," "articulated," and "hollowed" armour plating, faced and backed with all kinds of materials—such as wood, steel, sand, hides, india-rubber, wool, cotton, cocoa-nut fibre, cork, spiral springs, &c., &c.—was strongly recommended by one class of inventors. Another class preferred some other

protecting materials—wood, vulcanized india-rubber, cotton, &c.—instead of iron, and put forward schemes for using them. Others devoted attention to, what they thought, improved plans of fastening armour to the sides; and one inventor—a Frenchman—went so far as to propose to protect ships by movable plates, carried along on rails, and fixed temporarily where the attack of an enemy had to be resisted, "a sharp look-out being kept on the enemy's guns, to discover the point at which the plates would be required."

The simple plan of bolting slabs of iron to the sides of ships found favour with professional shipbuilders from the first, and has continued in use ever since. The question to which they devoted most attention was, What kind of ship would be best adapted to carrying armour? The floating batteries first built were not ship-shape in form or equipment, and were specially intended for attacking forts. 'La Gloire' was the first armoured sea-going *ship*. She was comparatively short and full, her ports were not high above water, and her rig was very light. Her construction was undoubtedly due to the proposals which the Emperor of the French had previously made to convert line-of-battle ships into iron-clad block-ships; and she proved moderately efficient in her sea-going capabilities, although the whole of her exposed broadside was covered with armour.

Our first iron-clad, the 'Warrior,' was designed on entirely different principles. She was made extremely long and fine; only a little more than half the exposed broadside was armoured, the extremities being quite unprotected; her ports were high out of water; and her rig was much heavier than 'La Gloire's,' so that she was capable of proceeding under sail alone. Her safety, therefore, depended largely upon the water-tight subdivision of the hold space at the extremities; and on this account many persons preferred 'La Gloire's' "complete" protection to the 'Warrior's' "partial" protection. In fact, after one or two modifications of the 'Warrior' plan, our designers actually adopted the system of complete protection in the converted ships of the 'Caledonia' class, and in the large ships of the 'Minotaur' class. In the latter all the other main features of the 'Warrior's' design were retained, and the strength of the armoured side was made about equal to the 'Warrior's,' but to do this a length of 400 feet, and a burden of more than 6600 tons were considered necessary. These were the last of the so-called "long" iron-clads built, and we are not likely to build any more; if we had kept on in the same course we should by this time have been building iron-clad 'Great Easterns,' costing a million or a million and a half each, to carry the thickness of armour some of our recent ships bear on their sides.

The contrast between these early vessels and our recent iron-clads, built during the last seven years, is very great. The 'Warrior,' for example, carries only 4½-inch armour on a burden of 6100 tons, while the 'Bellerophon' carries 6-inch armour on 4270 tons. The 'Minotaur,' with a burden of more than 6600 tons, carries 5½-inch armour; the 'Hercules,' of about 5200 tons, has 9, 8, and 6-inch plating, in addition to

special arrangements of backing at the water-line belt. Our latest vessels, the 'Thunderer' class of monitors, compare still more favourably with the 'Minotaur,' as they carry 12-inch armour on a burden of 4400 tons, and are impenetrable by the heaviest guns afloat. This superiority in the recent ships is due to various changes that will be referred to almost immediately; and the incentive to these rapid advances in thickness of armour is to be found in the increased weight and power of naval guns. In 1859 the 68-pounder cast-iron gun, weighing $4\frac{3}{4}$ tons, was our most powerful weapon: at present we have 600-pounder 25-ton guns, besides 18-ton, 12-ton, 9-ton, and $6\frac{1}{2}$-ton guns. The lightest of these is more powerful than the 68-pounder, and could pierce the 'Warrior's' side at 500 yards, while the old gun could not at 200 yards; and our 25-ton gun would probably pierce most iron-clads at a range of 2 miles. Heavier guns are talked of, and will probably be made soon; 30-ton guns are actually ordered; but so far as we are able to judge, our present guns are superior to any possessed by the French and Americans, and in strength of armour we certainly have a long lead. In fact, our iron-clad fleet now compares very favourably with the fleets of other nations.

The remarkable progress made in our iron-clad construction during the last seven years is mainly due to three causes:—Changes in forms and proportions; altered arrangements of armour; and structural changes. Recent ships are shorter and fuller than the 'Warrior' and 'Minotaur,' but they are quite as fast, or faster, notwithstanding. Our two fastest iron-clads, the 'Monarch' and 'Hercules,' are, in fact, examples of the short type. This question of speed was much debated at first; but, if experience can settle it, no further debate will take place. The reduction in dimensions in recent vessels has also given them wonderful handiness under steam as compared with their predecessors, and this has been still further increased by adoping the balanced or differential rudder. For example, on the official trials the 'Minotaur' took 7 minutes 38 seconds to traverse a circle 939 yards in diameter; while the 'Hercules' turned completely round in 4 minutes and traversed a circle only 562 yards in diameter.

The changes in the dispositions of armour on our iron-clads are also important. They chiefly consist in the adoption of the "belt-and-battery" system for broadside ships, and of the "breastwork" system for monitors. By means of these improvements the vital parts of iron-clads—such as the region of the water-line, the batteries or turrets in which the guns are fought, and the steering apparatus—are well protected; and the horizontal range of the guns carried behind armour is increased so as to give them an "all-round fire" instead of the very limited range possessed by the guns of the 'Warrior' and other early ships. These changes have been facilitated by the tendency of late years to reduce the number of the guns carried, and to considerably increase the power of each gun.

Not less important are the changes made in the structure of our recent iron-clads, for by these changes the ships have been made stronger and safer than their predecessors, while they have propor-

tionately lighter hulls. As an example, the cases of the early iron-clad 'Defence' and the recent iron-clad 'Invincible' may be taken. They are of nearly equal size; but while in the 'Defence' the hull weighs 3500 tons and carries something under 2500 tons, in the 'Invincible' a hull weighing less than 2700 tons carries more than 3200 tons. What is saved on hull can obviously be put into armour, and had the 'Defence' been built on the improved "bracket" system, she would have been able to carry twice as much armour as she actually does. Structural changes of this kind, combined with the changes previously referred to, have given us a fleet of powerful and handy ships, such as could not otherwise have been obtained; and have effected savings in expenditure that may be roundly stated at 100,000*l.* per ship for first-class iron-clads.

The changes recently made have had another important effect, which as yet has not been brought into prominence, although it deserves careful attention—*viz.* the much smaller straining forces short ships have to resist on account of their shortness and the concentration of their weights. Careful calculations show, for example, that for some positions afloat the 'Minotaur' may be strained *three* times as much as the 'Bellerophon'; and yet, to say the least, the shorter ship is as strong to resist these strains as the larger one. Taking the greater margin of strength of the short ship into account, and remembering the rapidity and violence of the changes of strain in ships pitching and tossing at sea—which, as it were, send *waves* of strain through the structure—it will be evident that the shorter type is likely to prove more durable than the longer, in addition to being superior in other respects.

A consideration which has justly had much weight in the reconstruction of our Navy, has been the necessity for giving most of our iron-clads sail-power, in addition to steam-power, so that they can keep the sea and cruise for considerable periods. This has also had much to do with the large adoption of the broadside principle in our ships as it is better adapted to full-rigged ships than the turret principle, on account of the fact that the horizontal range of the guns is not interfered with by the rigging, while the fire of turret-guns must be to some extent. In addition, the general requirements of a sea-going ship can be far better met in broadside ships. The turret principle may, of course, be carried out in rigged vessels, and has been applied to the 'Captain' and 'Monarch' with much success; but it is, in our opinion, much better suited to ships without masts or sails—such as our recent monitors—where the turret-guns have an all-round fire. These monitors are not cruising ships; but they have extremely large coal supplies, and, on an emergency, can proceed to the Mediterranean or cross the Atlantic. Wherever they go they will be free to fight without the encumbrance of masts or spars, and without the many risks in battle which rigging entails. Proposals to equip monitors of the American pattern—which is entirely different from our breast-work system—as sailing vessels have been made more than once; but

they have not been adopted because sufficient provisions for stores, equipment, accommodation, &c., have not been made, and (which is far more serious) because such vessels would run a great risk of upsetting when under canvas, on account of their deficient stability in inclined positions. Careful calculations have shown this to be the fact.

Iron-clad ships are very generally believed to be heavier rollers than unarmoured ships, because of the loads they carry on their top sides. Facts, however, prove them to be steadier than wood line-of-battle ships, and show that *raising* the weights on board tends to make ships roll less. The iron-clad two-deckers, 'Magenta' and 'Solferino,' are the steadiest French iron-clads. In designing some recent ships at the Admiralty, special care has actually been taken to raise weights in order to make the vessels steadier; modifications in the form may also be made to conduce to this result.

The burdens of armour carried by our ships are, of course, large; and though our iron-clads are called frigates, they are really heavier and larger than our largest three-deckers were. As experience has been gained and improvements have been made, we have gradually advanced in the proportion borne by the weight of armour to the total weight of the vessel. In the 'Warrior' the armour weighs less than one-ninth the total weight; in the 'Bellerophon' it slightly exceeds one-seventh; in the 'Hercules' it exceeds one-sixth; and in the monitor 'Thunderer' it considerably exceeds one-fourth. In fact, the 'Warrior,' with a total weight of about 9100 tons, carries only 975 tons of armour; while the 'Thunderer,' on a rather less displacement, carries 2370 tons of armour on sides, turrets, and decks. The monitor, as was said above, has no masts or sails, and is of only moderate speed as compared with the 'Warrior,' both of which causes help to increase the proportionate weight of armour.

The limit of thickness in armour has not yet been reached. Beginning with 4½-inch plates, we have in ten years reached 14-inch, and may go on to 20-inch, or even 24-inch plates—by means of improved arrangements not yet divulged—without making iron-clads of extravagant sizes. In fact the dimensions required for ships to carry the maximum thickness mentioned, need not exceed those of ships we actually possess. At present our strongest ships are impregnable to the heaviest guns, and as we can, when necessary, about double the thickness of their armour, it becomes very doubtful whether guns will soon be produced capable of beating the armour out of the field. It is much more probable that ramming and other modes of attack will take the place of artillery.

[E. J. R.]

GENERAL MONTHLY MEETING,

Monday, March 7, 1870.

Colonel PHILIP J. YORKE, F.R.S. in the Chair.

H.R.H. The Prince FREDERICK CHRISTIAN of Schleswig-Holstein, was *elected* an Honorary Member.

Wm. Henry Barlow, Esq. F.R.S.
Arthur John Booth, Esq.
Francis W. Buxton, Esq.
Joseph T. Clover, Esq. F.R.C.S.
Rev. John Congreve, M.A.
George Howard Darwin, Esq.
Francis Galton, Esq. F.R.S. F.G.S.
The Lord Ronald Sutherland Leveson Gower, M.P.
Robert Grubb, Esq.
John Gurney, Esq.
John O. Halloran, Esq.
Henry Hoare, Esq.
James Houldsworth, Esq.
Lieut.-Colonel Gordon Ives.
Thomas Jacomb, Esq.
Edmund C. Johnson, Esq.
Sir John J. Trevor Lawrence, Bt.
Frank McClean, Esq. M.A.
Hervey Pechell, Esq.
Frederick Pennington, Esq.
George Phillips, Esq.
Marlborough Robert Pryor, Esq.
John Rae, M.D.
Rev. David Mede Salter, M.A.
Albert George Sandeman, Esq.
George Stone, Esq.
Algernon A. De Lille Strickland, Esq.
Charles Bosworth Thurston, Esq. F.S.A.
The Hon. John Gage Prendergast Vereker.
Mrs. Michael Wills.
Henry Woods, Esq. M.P.

were *elected* Members of the Royal Institution.

The special thanks of the Members were returned for the following additions to "the Donation Fund for the Promotion of Experimental Researches":—

Arthur Giles Puller, Esq. (*4th Donation*) £21 0 0

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Actuaries, Institute of—Journal, No. 78. 8vo. 1870.
Agricultural Society of England, Royal—Journal, Second Series, No. 11. 8vo. 1870.
Asiatic Society, Royal—Journal, New Series, Vol. IV. Part 2. 8vo. 1870.

Asiatic Society of Bengal—Journal, No. 157. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. XX. No. 3. 8vo. 1870.
British Architects, Royal Institute of—Sessional Papers, 1869–70, No. 6. 4to.
British Museum Trustees—Catalogue of Diurnal Lepidoptera described by Fabricius. 8vo. 1869.
Chemical Society—Journal for Feb. 1870. 8vo.
Editors—Academy for Feb. 1870. 4to.
American Journal of Science for Jan. 1870. 8vo.
Artizan for Feb. 1870. 4to.
Athenæum for Feb 1870. 4to.
Chemical News for Feb. 1870. 4to.
Engineer for Feb. 1870. fol.
Horological Journal for Feb. 1870. 8vo.
Journal of Gas-Lighting for Feb. 1870. 4to.
Mechanics' Magazine for Feb. 1870. 8vo.
Nature for Feb. 1870. 4to.
Pharmaceutical Journal for Feb. 1870. 8vo.
Photographic News for Feb. 1870. 4to.
Practical Mechanics' Journal for Feb. 1870. 4to.
Revue des Cours Scientifiques et Littéraires. Feb. 1870. 4to.
Scientific Opinion. Feb. 1870. 4to.
Franklin Institute—Journal, Nos. 527, 528. 8vo. 1869.
Geological Institute, Imperial, Vienna—Jahrbuch, 1869. No. 3. 8vo.
Geological Society—Quarterly Journal, No. 101. 8vo. 1869.
Gladstone, John Hall, Esq. Ph.D. F.R.S. M.R.I. (*the Author*)—Papers on Phosphorus Compounds. 8vo.
Huddersfield Archæological Association—Yorkshire Archæological and Topographical Journal. Parts I. II. 8vo. 1869.
Meteorological Society—Proceedings, No. 46. 8vo. 1870.
Payne, J. Bertrand, Esq. M.R.I. (*the Editor*)—Haydn's Universal Index of Biography. 8vo. 1870.
Photographic Society—Journal, No. 214. 8vo. 1870.
Preussische Akademie der Wissenchaften—Monatsberichte, Dec. 1869. 8vo.
Radcliffe Trustees, Oxford—Second Radcliffe Catalogue of Stars. 8vo. 1870.
Royal Society of Edinburgh—Transactions. Vol. XXV. Part 2. 4to. 1868–9.
Proceedings. Nos. 77, 78, 79. 8vo. 1868–9.
Royal Society of London—Proceedings, No. 116. 8vo. 1870.
Symons, G. J. Esq. (*the Author*)—Symons' Monthly Meteorological Magazine, Feb. 1870. 8vo.
United Service Institution, Royal—Journal, No. 56. 8vo. 1869.
Vignoles, Charles B. Esq. F.R.S. (*the Author*)—Address as President of the Institution of Civil Engineers. 8vo. 1870.
Wechniakof, Theodore, Esq. (*the Author*)—Introduction aux Recherches sur l'Economie des Travaux Scientifiques et Esthétiques. (K 97) 8vo. 1870.

WEEKLY EVENING MEETING,

Friday, March 11, 1870.

George Busk, Esq. F.R.S. Vice-President, in the Chair.

Richard Westmacott, Esq. R.A. F.R.S.

A Chapter on Art.

Though selecting Art as the subject for consideration this evening, our remarks will not be directed to any special branch of practice. The purpose is to inquire, generally, into the condition and prospects of art among us at the present time. First, as to the interest felt in art by the public; secondly, on the capacity of the people to appreciate it; thirdly, on the opportunities for the people seeing works of art; and fourthly, whether these are of a kind to act beneficially, in an educational point of view, in elevating and improving the public taste. The term may also legitimately include the drama, as an art whose purpose is to address the intelligence and feelings through the sense.

The wide-spread exhibitions of paintings, not only in London, but all over the country, season after season, and the crowding of the public to visit them, afford sufficient evidence of the general interest felt in fine art. So, in another class of public entertainment. It is said that the same success attends the theatres, especially of the smaller class, which are nightly filled—the advertisements tell us—to overflowing. The capacity of the public to appreciate what is good, the character of the art provided, and its influence on public education, become then questions of some importance.

The art produced by any nation will be the expression or reflexion of the feeling of the people and of the age. The producers, or artists, being of the people, and living by the people, will, of course, supply the kind of art likely to be generally understood and patronized. Any even slight superiority in feeling may give rise to insulated examples of a higher class, but the exceptional genius or talent that produces this and the rare refinement able to appreciate it do not affect the main proposition. In like manner where the general national tone of art-feeling may be very low, there may be the imitation of better things. Sculpture may copy ancient statues, and illustrate obsolete Greek subjects, with which no one sympathizes; architecture may reproduce, to satiety, Grecian or Roman, or Italian, or Byzantine, or Gothic buildings; and painting supply imitations of mediæval, or what is called, not very intelligibly, pre-Raffaelite, art. But all this is quite distinct from a truthful national expression. Indeed, it rather proves the poverty of national or original art-feeling.

Any real value art may have must depend on its quality. This, in fine art, will be shown in its sentiment and intention, the character

of its subjects, and its making beauty a ruling condition. In its outward expression it will be exhibited in form, correct drawing, colour, harmonious effect, and all those qualities which constitute good art, in contradistinction to the merely mechanical.

The general tone or quality of art among us may be tested by a very simple process. At exhibitions of painting and sculpture take the trouble to mark on your catalogues the works which show the impress of high aim, deep thought, a marked feeling for beauty, correct drawing, and well-studied drapery; and compare with these the general staple of the productions exhibited. This will show what is the general feeling of the age, if only on the principle of supply answering to demand. In the drama, again. What is the ordinary quality of that produced to meet the public taste? We have been told by an experienced and accomplished writer and actor, that Shakespeare and the higher class of drama will not "draw," or pay, now-a-days. That the theatre is no longer sought for the intellectual enjoyment of witnessing a good play well performed, but that what is required and run after by the public is the highly-seasoned sensational drama, extravaganzas, burlesques, and especially showy spectâcle. Consequently the entertainment provided at many of these establishments is of a kind to preclude many families with daughters and children from attending them. The subjects, in too many instances, are the elaborate illustration of various phases of vice and crime; and even where a moral purpose is professed, the picture of immorality is so seductively painted that the positive representation alone fixes itself in the mind of the spectator. It cannot be said, in excuse for this preference, there is no good standard upon which a true judgment may be formed of what is right in these matters. We possess in England the finest examples of art, of the best time, and of the most accomplished masters of antiquity. There are also the productions of the great Italian and other schools in painting; and, putting aside the ancient writers, we have the works of Shakespeare and a long list of modern dramatists, from which ample examples of excellence may be drawn.

It has been said, most unjustly, that the blame of our inferior art rests with the artists. It is not the case. Artists, naturally, are influenced by the prevailing tone of their age, and having to live by their exertions, they must work to the public taste. But there are many who yearn for better things, and do, at great pecuniary sacrifice, produce better things; showing an ambition to raise the tone of art, which none, even of the educated classes, the noble, or the rich, ever appear to recognize, or dream of encouraging; and thus artists have to produce below their own standard or feeling in order to live. So in the drama. Is it not fair to believe that both authors and actors would prefer to make a reputation in a high rather than a low walk of their art? But the public will not have it. And why? Because there is no teaching or assistance afforded them to acquire a knowledge of, or feeling for, better things.

With respect to the opportunities for seeing art. There are in

London three great public institutions where works of art are collected. The South Kensington Museum, the National Gallery of paintings, and the British Museum, to all of which the public has, for the most part, free access. The first contains a vast collection of objects in fine art and in other branches of vertù, most admirably arranged for public inspection and enjoyment. The National Gallery contains a most valuable exhibition of pictures by the old and some modern masters; and here the public has a guarantee for the value of the collection, in the fact that the practical administration is under a highly accomplished professional artist, assisted by a secretary well known for his thorough acquaintance with the history and the masterpieces of art.

The British Museum is rich in its treasures of all kinds. Here, however, it is only to be considered with reference to our subject, Art. It possesses examples of the finest series of works in sculpture that exists, namely, those from the Parthenon at Athens, the production of Phidias and his scholars, and executed between 500 and 400 B.C. It would be impossible to describe adequately, at this time, the claims of these works to our admiration. Although mutilated, and stained with the dirt and rust of ages, and with the surface in many respects injured, they stand forth, by the verdict of all who are competent to judge of what is excellent in art, as the finest examples known of the true rendering of the most perfect forms in nature. Yet this seems to be very imperfectly felt by those in authority in our National Museum. The more important portions, the groups of the pediments, are so ill placed—their old and better arrangement, by a late eminent sculptor, having been entirely altered—that they cannot be properly seen; and scarcely ever is an artist found studying from them; and the uninformed public cannot in the least realize that there can be anything deserving attention in broken statues so distributed. Like all fine-art productions in the British Museum they seem to be simply included in the comprehensive category of Greek and Roman antiquities, and are, for the most part, housed as such. The Museum contains, in this department, a large collection of monuments. Among these, the remarkable sculptures from Assyria; the curious series of archaic seated statues from Miletus; sculptures of various dates from Lycia; some rilievi from Phigaleia, in Arcadia; and other examples of great interest in an antiquarian point of view especially deserve mention. Of a very important series of figures from the Temple of Jupiter Panhellenius, in Ægina, there are only casts, when we ought to have had the original marbles. This was owing to the commission to purchase them having been entrusted, not to a person conversant with art, but to the then superintendent of the department of Greek and Roman antiquities, who, though, possibly, an accomplished scholar, was, certainly, an exceedingly incompetent judge of even archaic art. He was sent abroad to secure them for this country; but, after the most cursory examination, decided against taking them. The entire collection was immediately purchased for Bavaria, for a much less sum of money

than England was ready to give for them. The originals are now in Munich.

A large collection of sculpture, rather lately acquired, deserves especial notice. It consists of fragments, almost innumerable, of statues, rilievi, horses, and other animals, and architectural remains from Halicarnassus; and their great interest is derived from their having formed part of the decoration of the far-famed monument erected by Artemisia, about 355 B.C., in memory of her husband, Mausolus. Some portions of the rilievi of this collection, presents from the Sultan to our ambassador, Sir Stratford Canning, had reached this country as far back as 1846. The real site of the Mausoleum was not, however, ascertained till many years later, and here was found the immense accumulation of fragments we now possess. The greatest expectations were indulged when this discovery was announced, and as Vitruvius mentions that Praxiteles, and Pliny that Scopas, were among the sculptors employed in its decoration, it was reasonably hoped that some works of transcendent merit would be brought to light. It is mortifying that, in no respect, have any of these anticipations been realized. It would be an insult to the fame of Praxiteles to attribute any of the sculpture now recovered to his master-hand. The various works may be said generally to bear the traditional character of a good school, as might be expected from the date of their production, when sculpture had attained its supreme perfection; but they vary in merit; they are very incomplete and unfinished in their execution, and seldom exhibit any really fine detail. Indeed it may be said that there is not among them a single example of such excellence that any competent judge of art could conscientiously recommend a pupil or young student to take it as his model; nor indeed is any artist ever seen making studies from them. The objects arrived in this country in a state of dreadful dilapidation. Many of them have been repaired, as far as possible, to make them intelligible. Of these sculptures the most remarkable is a colossal draped male figure. In putting this together no fewer than from sixty to seventy fragments were found and fitted; and yet it is still very far from complete. The head is not an integral part of the statue. There is a hollow, or worked socket in the statue made to receive a plug, and a head was found among the fragments with this appendage to it. It did not, however, quite fit into the space, when, curiously enough, a layer of ancient lead, about half-an-inch thick, was found, which had evidently been run in to make up the joint; and thus the appropriation of the head was established. This figure, though somewhat clumsy and heavy in its form, and short in its proportions, is simply composed, and of a grand and commanding character. There is no nude or naked portion exhibited, nor any extremities, with the exception of one sandalled foot, from which to form a conjecture of the action, nor any accessory to suggest its subject. It is entirely covered, from the throat, with its drapery, which is ample and well composed. The execution, especially at the back of the figure, is very incomplete. The

head is striking for its expression of calm dignity; but the features, though not wanting in beauty, are out of drawing, and the hair and beard are left in a most rough and unfinished state. The statue is now exhibited as a supposed portrait of Mausolus. This is purely conjectural. There is not the slightest authority for it. There is no portrait known of Mausolus; and at the date of these sculptures portraiture was not practised; nor at all, it is believed, before the later time of Alexander the Great. Besides, as has been shown, the head was an *appliqué* head, and under no circumstances can it be fully accepted as an integral part of the original statue, whoever it may have been intended to represent.

The rilievi are of various degrees of merit; many of them are well composed, and the movement of the groups spirited; but in richness and variety of action they are very inferior to those of a similar subject from Phigaleia. The portions of colossal horses are of a very coarse and clumsy type, and have the character rather of some of the heavy Roman sculpture than of the finer Greek school. In all respects they contrast very unfavourably with the noble examples of the class in the Elgin sculptures. Of other animals, some are sketched with spirit and with truth of expression, but all are very unfinished.

The Mausoleum was overthrown by an earthquake, which will account in a great measure for the sculpture being so utterly broken up; but all these works bear unmistakable evidence of haste and incompleteness. The history of the Mausoleum suggests sufficient reasons for this. As we do not find that there was a school of art at Halicarnassus, Artemisia probably sent her emissaries to procure works and workmen for this monument wherever they could be found; and, chiefly intent on a sort of barbaric magnificence, collected materials for its enrichment with but little regard to fitness. This seems certainly to be the case, from the great mixture of objects and their varied design. Among other reasons for holding this opinion, open of course to correction, the subjects of the rilievi—the contest of Athenians and Amazons—do not, as far as is known, appear to have any connection with the history of Mausolus. The sculptures throughout bear the evidence of being by several different hands, as the traditional history of the work declares; and it is probable many of them were only prepared, and intended to be finished after they were placed—not an uncommon practice with the ancients. But Artemisia is said to have died in the year after her husband, and if this were the case, no doubt the works were never wholly finished in the manner originally contemplated. The names of the different sculptors employed are recorded by Vitruvius and by Pliny, though one of these omits Praxiteles, and the other Scopas, from his list. Those recorded by both are not artists of any great reputation, and the respective works lead to this conclusion. It must, however, be remembered that the above authors lived between 300 and 400 years after the date of this work, and the authenticity of the traditions respecting the Mausoleum must, by that time, have been greatly impaired.

Shiploads of these mutilated—and, taken collectively, indifferent—sculptures, and even large stones or blocks which formed the building, were sent to England, at, of course, an enormous cost, not yet ended; for accommodation has to be found for them. Unfortunately they add nothing in value to our objects of high art, nor much to our character as judicious collectors. They are interesting for their historical associations, but not as examples of good sculpture. A well-chosen selection from the mass of *débris* would have answered every purpose; but no competent adviser seems to have been sent out by the British Museum to examine and report upon the artistic merits of the works, and even the respected superintendent of the department of Greek and Roman antiquities at that time (Mr. Hawkins) knew little or nothing about them till they reached England and were placed in his charge.

It is to be lamented that a division of the public art possessions so important as Greek sculpture, should be so inadequately provided for—but so it is. The Board of Trustees is composed of noblemen and gentlemen of high position and character, many of literary eminence, some of scientific attainments, and all doubtless desirous to act for the benefit of the institution; but, unhappily, none among them have, or profess to have, such a knowledge of art as will give confidence in their unassisted judgment on such subjects; and thus, from never having made them their study, they have never been in a position to act independently of official pretension and dictation. The officers in the different departments of the Museum, as in that of antiquities generally, in coins and medals, in the library and print-room, and in science, are gentlemen of accomplishment in their own special walks; but, while a superintendent of antiquities may be a valuable officer within the range and limits of his special duties, he is not, necessarily, a safe judge of fine art. There is no one of recognized competency to whom professional artists and the public can confidently refer; and no tribunal or jury, as there was in the case of purchasing the Elgin sculptures, to advise the Trustees in such questions. From the first establishment of the Museum, the officer who has the care of the antiquities, has been supposed, from his having the charge of the sculpture, to be, also, intuitively a judge of sculpture, when, from the want of any practical education in art, he may only be competent to estimate it as an antiquary. Hence some of the mistakes that might easily be exposed of injudicious selections, of extravagant sums paid for very indifferent works, or the loss, like that already referred to, of desirable acquisitions.

No doubt it is essential that the officers in this department should be men of some learning. Professional artists, not usually being so, would be quite unfitted for such situations; besides, no artist of any position, or fond of his pursuit, would care to give up his time to become a *custode* in a museum. Still the importance of practical acquaintance with art, a knowledge of anatomy, human and comparative, of characteristic, or beautiful and select form, and the refinements

of execution, are essential where sculpture is to be judged as an exercise of fine art, and not merely from an antiquary's contracted point of view. The proper exhibition of such works, their position, elevation, the light they are to receive, are all matters for artistic consideration, and, without disparagement, they are beyond the competency of anyone who has not been properly trained to deal with such subjects.

There can be no doubt that a very large amount of sculptured monuments have been admitted into our national collection that ought never to have had a place there; and which never would have had a place there had there been anything like a competent tribunal to decide upon their value. Specimens would, in many instances, have answered every purpose, where space has now been sacrificed to accommodate piled up accumulations of what is little better, harshly as it may sound, than antiquarian rubbish. It is not too late while changes, involving great expense, are being made for arranging some of these objects, to have the whole collection surveyed by a competent jury of artists and antiquaries. The usefulness of the Museum would be increased, and the character of the country, in its judgment of fine art, would be greatly advanced if the sculpture galleries of the British Museum were subjected to a careful and judicious weeding.

It is but justice to say that, in the importance of its possessions in archæological monuments, the British Museum may challenge comparison with any collection in the world. The previous observations are not intended to convey any reflection on the acquisition, within reasonable limits, of such illustrations of the history, religion, poetry, customs of ancient nations, nor on the value of the services of those whose province it is to elucidate the difficult and interesting problems offered by these relics of the past. The strictures, if they may be so termed, are addressed to the mistaken system of confusing two entirely distinct objects, and thus including fine sculpture, essentially and perhaps solely valuable for its qualities as fine art, and to be studied as a means of education and admired for its objective excellence, in the same category as sculpture which may be of the most rude and archaic kind, and without the most remote pretension to rank as art. The effect of the former, and the advantage to be derived from it, in an educational sense, are greatly impaired by the juxtaposition of often unsightly and grotesque objects, which may only be worth preserving for their possible antiquarian value. Our possessions in really fine works of sculpture are not very considerable; but we have some of very fair pretensions, and, with these, a few examples of the highest class. None of these are made the most of; or it may be more truly said they are almost lost sight of, and as it were overlaid by the accumulation around them of works of no artistic merit. Where the antiquarian element is allowed to prevail so extensively as it now does in the British Museum, the present objectionable system is likely to continue. It may be hoped, however, that the time may arrive when the two objects of fine art and archæology may be better under-

stood and more distinctly defined, and that, without ignoring the importance of the latter, the exhibition of the finest productions of the taste, the knowledge, and the skill of the great masters of sculpture may be allowed to take their proper position. But there can be no doubt a more judicious arrangement than the existing one would give much greater effect to the better class of works, and would add to their usefulness. It is in this spirit that the fine-art portion of the national collections should always be treated, if, as is contended, the purpose of making such collections is to endeavour to attract public attention to the beauty and value of art, and, through such works, to study and appreciate the true source of their excellence.

But galleries and museums can, after all, do but little in the way of educating the people in art if they have not some assistance to help them how to see whether and why the objects so presented to them are worthy or unworthy. For this reason it is to be regretted that some sort of lectures cannot occasionally be given, by competent persons, to point out in what the excellence of certain works, whether in painting or sculpture, consists; to explain the peculiarities of the different schools of painting; to illustrate, by reference to the particular objects, the leading characteristics of the great masters in all branches of art; and so to teach the public how to look at and appreciate Nature, as the only perfect guide and standard the real artist aspires to follow.

It is not proposed, even if it were possible, to make all people artists; but all may be taught how to judge of the leading principles of art, and thus to increase their means of enjoyment. Such power may be said to impart to its possessor a new faculty; for it is in the experience of every one, more or less, that even a slight acquaintance with the principles of any science or art is a great aid in enabling us to appreciate it. Looking at art is not always seeing it, so to speak. An uneducated eye would not distinguish, as a cultivated sense would, between a mere commonplace view of fields, trees, and sky, and the exquisite and varied rendering of the subject by a Claude, a Hobbima, or our own poetical Turner; precisely as the uncultivated and unpractised ear would fail to appreciate the difference between a common tune played on a dismal street-organ and the sweet fancy or the grand combinations of a composition by a Beethoven or a Mendelssohn.

[R. W.]

WEEKLY EVENING MEETING,

Friday, March 18, 1870.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

JOHN FREDERIC BATEMAN, Esq. F.R.S. C.E. M.R.I.

On the Subway to France.

THE advantages which a Railway communication between England and France would ensure to Western Europe, are great beyond all possibility of estimate. Thirty years ago any proposition with a view to accomplish this object would have been looked upon as wholly Utopian. But we are living in an age of invention and progress which within one century has had most beneficial effects upon our race, having not only greatly increased the welfare and comfort of millions of our fellow-creatures, but also equally widened the field of inquiry. New discoveries have rapidly followed one another, enlarging our knowledge and extending our conceptions, all tending to elevate the mind of man, and to raise the standard of his moral and intellectual being. It is just one hundred years since Watt invented the steam-engine—an invention which has done more than any other to promote the commercial prosperity of all countries, and to develop their resources. It is only forty years ago that the Manchester and Liverpool Railway was opened, and since then every civilized country has been covered by a network of iron ways; it is not more than sixty years since the first cast-iron pipes were used for conveying water; fifty years back none of our cities had the inestimable advantage of gas-light; it is but thirty years since the first steamer crossed the Atlantic, and in less than thirty years our globe has been girdled with a band of lightning, which unites its most distant parts with inconceivable velocity, annihilating space, and flashing from one end to the other the thoughts and ideas of men,—defying even the speed of time.

Looking at these facts, the speaker expressed a hope that he would not be considered a visionary schemer in proposing to cross the English Channel by a Railway. The proposition itself is not a new one. There are some who propose to construct a bridge for carrying a Railway; others, to drive a tunnel through the rocky bed of the Channel; and, again, others who propose to *sink* covered subways—all with a view to

unite by a direct Railway communication the shores of England and France.

The effect which such a communication would produce on the traffic between the two countries may be foreshadowed by the events of the past. Forty years ago, in the good old sailing times, the number of passengers between this country and the Continent was 80,000 per annum; the introduction of a regular steam-boat service raised this figure in twelve years to 350,000; and since the construction of Railways it has further risen to upwards of a million. Increased facilities and convenience have therefore greatly extended the traffic between the two countries. Nor should it be overlooked how much that enemy of ocean travellers—sea-sickness—is checking its further extension. We have but to look to the official return of travellers between England and the ports of Calais, Boulogne, and Dieppe, to observe, that in the year 1867, for example, the number of passengers in the fine month of August was 80,000, whilst in the stormy season of January and February it only reached 13,000 in each month; and although the season of the year may have somewhat affected the figures, apart from bad weather in the Channel, it is obvious that so great a difference will be mainly due to the dread of sea-sickness, to which most travellers are liable in rough weather.

Referring to the communication with India and the East, Captain Tylor, in a recent Report to the Board of Trade on the advantages of the various routes across Europe, observes, "the hope of the Indian traveller of the next generation, chimerical as it may appear to many at present, must be nothing less than to pass dry shod from London to Bombay, through a submarine tunnel from Dover to Calais, through Europe by Railway; over the Bosphorus by a bridge, and forward by Railway by the Euphrates Valley, and round the Persian Gulf, to Bombay."

The speaker observed that it was not his business here to make any observations upon the schemes of others, contenting himself to submit and describe a plan designed and elaborated jointly by himself and his colleague, Mr. Révy. He considered it especially his task to suggest every possible difficulty—real or imaginary; and it was not until all these difficulties, one by one, had been successfully overcome by their joint exertions extending over more than a year, that they had published their scheme of which he now proposed to give a short description. Their plan was to build a cast-iron tube on the bottom of the Channel between Dover and Cape Grisnez in France, large enough to carry ordinary trains; the tube will be cylindrical, 13 feet clear diameter inside, and will be composed of cast-iron rings about 10 feet long and 4 inches thick; each ring again will be formed of six plates or segments, securely bolted to one another. The various parts of the tube will be put together from inside a horizontal chamber (for convenience called a Bell), which will slide on the last half-dozen completed rings of the tube over its external ribs or diaphragms, in which a water-tight joint will completely prevent the sea water from

entering. This water-tight joint will be similar to that so extensively used for hydraulic presses. In the exterior rib of the tube, closely fitting the inner surface of the chamber, a groove will be cut all round, about 1 inch deep and 1½ inch wide. Into this groove a leather or india-rubber collar will be placed, so adjusted that the greater the pressure the tighter the joint.

These points will be the only points of contact, and over them the bell or chamber will slide like a telescopic tube. Four of these joints will always be in the bell, so that a slight imperfection in the water-tightness of one may be corrected by another.

The dimensions of the chamber, which will be permanently closed in front by a cover, will be about 80 feet long, 18 feet diameter clear inside, and 8 inches thick; it will be bored out for about 50 feet to a true cylindrical surface, namely, for a distance within which it is in contact with and will slide over the external ribs of the tube, which will also be faced and turned for the purpose of securing accuracy of fitting.

In the foremost part of the chamber, or bell, will be placed powerful hydraulic machinery, receiving the water from accumulators on shore, upon Sir W. Armstrong's system. By the aid of this machinery the tube will be put together from inside the bell, and as the tube lengthens by the addition of another ring, the bell will be pushed forward by hydraulic presses for about 10 feet, to make room for the construction of the next following ring—always using the completed portion of the tube as a fulcrum for moving forward the bell, which in the deepest part of the Channel will meet with a hydrostatic resistance of about 1500 tons. The power of the hydraulic presses will be over 4000 tons, of which the balance of 2500 will be available to overcome any resistance with which the bell in its onward motion may meet on the bottom of the Channel.

The displacement of water by the bell and by the tube will be nearly the same as their respective weights—weight being kept slightly in excess of displacement—so that the tube and the bell will weigh but very little under water.

In the annular space between the exterior tubular ribs of the tube and the inner surface of the bell, the head of a screw pile may be introduced, or several of them, as may be required. These piles may be attached to any ring on the line of the tube. The shaft of the pile will pass through a stuffing-box in the ring, like the rod of a piston in a steam-cylinder. On the average every third ring will receive one vertical pile, and every sixth two piles 30° inclined. As soon as the bell in its onward motion has passed over the rings in which the screws of the piles were introduced, and the shafts have been attached, the piles will be driven or screwed into the ground until a firm hold or footing is obtained. By these means the tube will be permanently tied and powerfully held to the bottom of the Channel, which nothing afterwards could alter or disturb.

The ordinary construction of rings will keep the tube in a straight

line; but by using special castings and moveable joints or diaphragms in connection with the outer rib of the tube, the direction of the bell may be altered in every 40 feet about 4 inches, corresponding to a curve of about a mile radius. The tube may accordingly also be laid on a curved line in any direction; it may be made to rise or fall 36 feet in a distance of 200 yards; and it may be turned round at right angles with its former direction within one mile. It may thus either be turned away altogether from a serious obstruction—such as a sunken rock or soft ground, which a detailed survey on the line selected might bring to light—or it may pass over the obstruction, or follow a sudden and extensive depression in the bed of the Channel. By reversing the operations within the bell, and by taking down the segments and rings of the tube already built, it may for a considerable distance be taken up again, in case another direction should be required at a point of the line already constructed.

The operation of building the tube on shore will be attended with more difficulty than in deep water; because during the construction of the shore-ends the whole weight of the bell, the machinery, and of part of the tube will have to be supported and carried on slips, while in deep water these weights will be counterbalanced by the floating power of the bell. These slips will be used until low-water line be reached, at which point the operations will be confined to high-water time; and the bell having by degrees advanced far enough into the sea, so that it will be wholly covered even at low water—the operation of building may be continued without intermission. A similar proceeding in the reversed order will be adopted on reaching the French shore. During the construction of the shore-ends in rough weather and a heavy sea, the bell will temporarily also be firmly held in its position by screw piling.

All the parts of the tube will be constructed and completed and will be temporarily put together on shore, before they are sent into the far-end of the tube. They will, on their arrival there, be lowered on rails, from which the segments will be picked up, placed and fixed in their final position by special hydraulic machinery, all of which has been designed in detail, and no manual labour whatever will be required for this purpose. The parts of the tube having already been put together on shore, there can be neither delay nor difficulty in repeating the operation by the aid of powerful hydraulic machinery, and accordingly the progress of building the tube from inside the bell should be rapid. Special pipes will convey from shore, by mechanical power, fresh air to the workmen, and water to the machinery to the far-end of the tube. The business of the workmen will chiefly be to direct the machinery, to open and close certain valves, to insert the bolts, to caulk the lead joints, &c., all the heavy work being accomplished by special machinery. The rings of the tube having been constructed, a brick lining will be introduced between the inner flanges; and over this brick lining a wrought-iron plate will be bedded in cement, and this plate will form the inner surface of the finished tube. All the lead joints will, however, remain exposed to view, and should a drop of

water leak through any joint at any point within the tube, it may be seen, and may be caulked again until that very drop be excluded. The tube when finished will be perfectly dry from shore to shore.

The effect of sea water on cast iron has also been carefully considered—it is practically "nil" within a generation. The cast-iron guns of the 'Royal George,' after having been submerged for 52 years in the tidal waters of the Channel, were recovered from the wreck in the year 1834 wholly uninjured and scarcely altered in their appearance. Specimens of these guns may be seen in the Tower of London; even their bore is in perfect condition. Neither anchors nor wrecks of any kind could make the slightest impression on the tube, for it would be by far the strongest structure.

Of equal importance is the consideration how such a tube or tunnel when completed, may be made to carry a train from England to France. It will presently be seen, that for the propulsion of trains locomotive engines could not be employed in such a long tunnel, because the combustion of the fuel of the engine would so vitiate and poison the atmosphere within it, that the passenger would be in danger, and because the ventilation of such a very long tunnel is a matter of difficulty.

In order to warrant the execution of such extraordinary works, the carrying power of the proposed Railway should not be less than about two trains per hour, one each way. An engine would burn about half a ton of coke in passing through the tube with a heavy train, so that per hour about one ton of coke would be burnt; and even assuming the most perfect combustion, more than three tons of carbonic acid would be produced. To neutralize the dangerous effect of this gas it should be diluted with at least 500 tons of fresh air. By some means this quantity of air must be introduced, which would require a constant current to be maintained within the tube at the rate of about 20 miles an hour, to produce which a difference of pressure equal to $3\frac{1}{3}$rd inches mercury would be necessary between the two ends of the tube. Such a pressure could only be obtained by air-pumps; and considering the great volume of air to be displaced, their dimensions would be very large. This difficulty has, however, been met by adopting air-pumps in the form of gasometers, in which a cylindrical shell, raised and lowered by mechanical power, would easily displace a practically unlimited volume of air at the required pressure. An additional inch pressure of mercury over the large sectional area of the tube, brought to bear on a disc attached to the train, would produce a traction greater than that of the heaviest engine. Trains might therefore be propelled through the tube by pneumatic pressure without engines, doing away altogether with the nuisance and danger which the combustion of fuel would inevitably produce.

Atmospheric pressure, it is well known, has been used for years in working railways, and the speaker observed that he had travelled on a line so worked at a rate of 60 miles an hour. The old atmospheric plan of propulsion was, however, abandoned, because of the mecha-

nical difficulty of connecting the train with the small piston of the atmospheric tube, only about 15 inches in diameter, which was laid *under* the train, and the high pressure of air required for propulsion. In the present case, these difficulties disappear, because the train will travel *within* the tube with a piston of 13 feet in diameter, requiring but a small pressure per square inch. At a speed of 20 miles an hour and a transmission of 12,000 tons of goods per day—6000 each way—the difference of pressure between the two shore-ends of the tube would be $4\frac{1}{2}$ inches mercury, equal to about 2 lbs. per square inch. With the same daily transmission of goods and a speed of 30 miles an hour, the difference of pressure would be $8\frac{1}{4}$ inches mercury, or about 4 lbs. per square inch. It has been found by calculation that the promptest and most economical way of producing these differences of pressure would be half by compression and half by expansion, *viz.* forcing the air into the tube behind the train, and withdrawing it in front of it. The passengers would not be aware of any difference of pressure in the condition of the atmosphere while travelling through the tube. At one end of the tunnel the atmospheric pressure would be similar to that of a deep mine,—at the other end similar to the rarefied air upon a high mountain, like Snowdon or Ben Lomond. The indicated horse-power to work the above-mentioned traffic of goods per day, at a rate of 20 miles an hour, would be about 2300; at a rate of 30 miles, it would be 6300—divided equally between the steam-engine establishments on each coast.

By adopting the Pneumatic System of propulsion serious accidents would be out of the question: no collision could take place, and the pure sea air would accompany every train, securing perfect ventilation. The position of the train within the tube would always be registered on shore by self-acting telegraphic signals; moreover, telegraphic messages may be sent from the train while at full speed to either shore-end, so that the passengers would be in immediate communication with those on shore. If a train should break down during its passage through the tube from some cause or other, it would be instantly known on shore, and, if necessary, an ordinary train with an engine might be sent for relief. The objection to locomotive engines to work the traffic arises only from the required continuous passage of engines vitiating by degrees the atmosphere; but an occasional engine and train would in no way affect the passengers, for the pumps would have to remove all the tainted air before another train was allowed to pass.

On the proposed plan, the railway across the English Channel might be completed in less than five years at a cost of 8,000,000*l.* sterling, allowing interest on capital expended during construction of works and 1,000,000*l.* for contingencies.

The speaker said, in conclusion, that he believed such a railway would be a most successful commercial enterprise. There are, he observed, three conditions which ensure the success of an undertaking, *viz.*—1st, it should be a material improvement on the existing state of things; 2nd, its want should be recognized by the public; 3rd, it

should be placed in the right hands, and it should be under absolute control. The chief merit of the scheme, if any, might be the circumstance that no part of it depended on "ifs," and that the whole of the scheme is as much under control as ordinary mechanical operations. The cost could be estimated with a great degree of certainty, which could scarcely be claimed in respect of any other proposal. It would consist nearly wholly of cast iron and machinery on shore, the only uncertain item being the cost of putting the tube together under water. There seemed to be no doubt that the bottom of the Channel consists of good hard ground, as indicated on the Admiralty Charts; but he had recently applied to the Admiralty for a more minute and detailed survey of part of the English Channel, and he had reason to hope his request would be granted.

[Numerous diagrams were exhibited, illustrating the principal parts of the scheme and its more important details.]

[J. F. B.

WEEKLY EVENING MEETING,

Friday, March 25, 1870.

JOHN PETER GASSIOT, Esq. F.R.S. Vice-President, in the Chair.

GEORGE ROLLESTON, Esq. M.D. F.R.S.

On the Character and Influence of the Anglo-Saxon Conquest of England, as illustrated by Archæological Research.

THERE are numerous points of general and living interest relating to the Anglo-Saxon conquest of this country which are very largely dependent upon archæological research for their elucidation. Amongst these may be mentioned the question of the extent to which the Romano-British population previously in occupation was extirpated; the question of the relative position, in the scale of civilization, held by victors and vanquished; and the question of the extent of our indebtedness as to language and laws to one or other of the two nationalities. Light is thrown even upon points apparently of the most purely archæological character from such literary sources as histories of the nomenclature of localities; as the records of monasteries; as illustrations in manuscripts; and as laws. But the graves of the Anglo-Saxons and their contents have been for the present investigator the primary, and such literary works as those alluded to, and such as many of those published under the direction of the Master of the Rolls and by the Early English Society, have been only a secondary source of information. They have, however, been by no means neglected by him.

It may be well to begin by stating how an Anglo-Saxon is to be distinguished from a Romano-British interment. Anglo-Saxons, during the period of their heathendom, which may be spoken of roughly as corresponding in England to a period of some 200 or 230 years onwards from their first invasion of the country in force, were interred in the way of cremation, and in urns of the pattern so common in the parts of North Germany and of Denmark whence they are supposed on all hands to have come. A reference to any manual of archæology, or an inspection of any such series as that figured by Mr. Kemble in the *Horæ Ferales* from the Museum in Hanover, will show the unmistakable identity of the pattern, fashion, and moulding of such urns as these, and these which I have had figured after digging them up in Berkshire. The Romans and Romano-Britons had given up the practice of burning the dead long before the time of Hengist and Horsa. When they practised it in England, their urns were of a very different kind, being well burnt and lathe-turned. All the Romano-Britons I have exhumed in the particular cemetery which has furnished me with the tolerably wide basis of something approaching to 200 interments of all kinds, were interred much as we inter our dead now. They were oriented, though by the aid of the sun and not by that of a compass; and, dying in greater numbers in the winter quarters of the years, had the bearings of their graves, as has been observed by the Abbé Cochet, pointing a little south of east. Now a Romano-British interment in this way of burial has to be distinguished from an Anglo-Saxon one in the same way of non-cremation; and this may be done thus. The Romano-Britons never buried arms nor any other implements which could be of use in this, and might be supposed to be of similar use in the next world, together with a corpse. Funeral ware, such as lacrymatories, I have not found in company with coins of the Christian Emperors; but such articles stand in relation to quite a different idea from that which caused the Teuton to inter the dead with spear, shield, and knife; to say nothing of the less common *situla* and sword.

The Anglo-Saxons are supposed by Kemble to have relinquished cremation only when they assumed Christianity; it is a little difficult to be quite sure of this; at any rate, when we find, as we often do, an Anglo-Saxon in a very shallow grave, which may point to any one point of the compass, and in the arms or other insignia which it contains, gives us such clear proof that its tenant thought that whatever he may *not* have brought with him into the world, at all events he could take *something* out, we are tempted to differ even from such authority as Mr. Kemble's. But I am inclined to think that in some cases it is possible to identify the tenant of a properly oriented grave as having been an Anglo-Saxon. In many such graves Anglo-Saxons are to be recognized by virtue of their insignia; and mixed up with their bones may be found the bones of the Romano-Briton who occupied the grave before them. But further, in some such cases it is possible to be nearly sure that we have to deal with an Anglo-Saxon even though there be no arms or insignia in the grave. These cases are those in which we

have evidence from the presence of stones under the skull that no coffin was employed in the burial; and in which stones are set alongside of the grave as if vicariously. In many such cases the craniological character of the occupant of such a grave lends some colour to this supposition. But upon such identifications as had been come to in the absence of arms and insignia I have based no statistics. The results of the statistics of the cemetery which I have explored, as stated above, when brought to bear upon the large questions alluded to at the beginning of the paper, would lead us to think that the Anglo-Saxons were in a considerably lower grade of civilization than the people they conquered, firstly and most forcibly on account of the shorter lives they led. An old Anglo-Saxon male skeleton was a rarity, an old Romano-British one a very common "find" in my excavations. Nothing however in this life is from the natural history point of view more characteristic of real civilization or real savagery than this matter of the duration of life. The Merovingian Franks had, like the followers of Cerdic, been observed to have led short lives, merry, as the Capitularies of Charlemagne teach us of their kinsmen, with those kinds of mirth the end of which is heaviness. The next question which suggests itself upon the mastery of these facts and figures is, were not these men merely soldiers encamped? are not these statistics just such as a cemetery similarly explored now-a-days, say at Peshawur or Samarcand, would yield? Not altogether such; for, however improbable it may seem, it is nevertheless true that the Anglo-Saxons, at all events in Berkshire, appear to have brought their own wives with them and not to have provided themselves with wives from the families of the conquered previous inhabitants. The figures of the crania of females interred with Anglo-Saxon insignia, when compared with figures of the crania of Romano-British women, show a very great difference, to the disadvantage of the former of the two sets of females. The soldiers of Cerdic, who conquered this part of Berkshire about half-a-century or so after the time of the first invasion, resembled the soldiers of Gustavus Adolphus in very little else, but they appear to have resembled them in being accompanied by their wives. Whether this was the case elsewhere in England, I do not know; I am inclined to think that savagery was no great recommendation, nor heathendom either, to a Christianized female population in those days; and that the reluctance which would on these grounds interpose itself to prevent intermarriage between Romano-Britons and Saxons, sets up as great an *a priori* improbability against the theory which assumes that such inter-marriages did take place, as the difficulty of bringing wives over in the ships of those days sets up in its favour.

Indeed, on the hypothesis of much inter-marriage the actuality of our Anglo-Saxon language is a very great difficulty. We do speak a language which, though containing much Celtic and a good deal of Norman-French, is nevertheless "English." Now we know, from finding cremation urns of the Anglo-Saxon type all over England nearly, that the whole of the country was overrun by a heathen popula-

tion; to thus overrun it, this population must have been (relatively at least) numerous: add to the two conditions of heathendom and multitude, which may be considered as proved, the third condition of isolation which may be considered as matter for dispute; and then the fourth of this heathendom and isolation lasting from the time of Hengist to that of Augustine; and the present fact of our language being what it is is explained.

For proving anything as to the period of which I have been speaking, a period which is rendered Pre-historic, not so much by conditions of time as by conditions of space, the absence of contemporary historians having been entailed by geographical and political isolation, arguments of two kinds, literary arguments and natural history arguments, must be employed. Neither the one kind nor the other is sufficient by itself. The empires of the natural sciences and of literature touch at many isolated points, and here and there they lie alongside of each other along lengthy boundary lines. But empires need not be hostile though they be conterminous; and that the empires of which we have just spoken may be united happily and in a most efficient alliance for work in common, may be seen from the title page of that most excellent German periodical, the 'Archiv für Anthropologie,' where we have the name of the Physiologist Ecker coupled in editorship with that of the Antiquarian Lindenschmit. The necessity for a combination of the two lines of evidence and argument is as obvious when we have to controvert, as when we have to establish a conclusion. If you have to attack or resist a force comprising both cavalry and infantry, you must have both cavalry and infantry of your own; otherwise some day or other, either in a country intersected with woods, or in some open plain furrowed into deep undulations, one of the two arms in which you are deficient will take you in one or both flanks, and you will be surprised, broken, and routed.

[G. R.]

WEEKLY EVENING MEETING,

Friday, April 1, 1870.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

PROFESSOR H. E. ROSCOE, F.R.S.
OWENS COLLEGE, MANCHESTER.

On the Artificial Production of Alizarine, the Colouring Principle of Madder.

THE speaker stated that he had to bring before the notice of his audience a discovery in organic chemistry which, whether we regard its scientific interest or its practical and commercial value, is of the highest importance, and marks an era in the history of the application of chemistry to the arts and manufactures even of greater importance than the memorable discovery made by Mr. Perkin in 1856 of the production of aniline-violet, or mauve.

Since the above-named year great progress has been made in the theoretical investigation of natural and artificial colouring matters as well as in their preparation on a large scale. The chemistry of colouring matters has now taken a high and important position, and chemists instead, as formerly was their wont, of getting rid of all colouring matters as something foreign to their objects of investigation, have, since Mr. Perkin's discovery, found out that the examination of colouring matters may not only lead to scientific laurels, but may sometimes yield fruit of another and not less acceptable kind.

We owe to the brains and hands of two German chemists, Messrs. Graebe and Liebermann, this remarkable discovery, which differs from all the former results which have been brought about by the application of science to the chemistry of colouring matters, inasmuch as this has reference to the artificial production of a natural vegetable colouring substance which has been used as a dye from time immemorial, and is still employed in enormous quantities for the production of the pink, purple, and black colours which are seen everywhere on printed calicoes, viz. *Alizarine*, the colouring principle of *Madder*.

It is from the liquid tarry products of the destructive distillation

of coal, a rich source of interest to chemists, that we now derive this new colouring matter.

The following Table contains the results of experiments made on the large scale, indicating the various yields of tar from different qualities of coal distilled in the gasworks of various towns:—

DESTRUCTIVE DISTILLATION OF COAL.

100 tons of Cannel and Bituminous Coal distilled to yield 10,000 cubic feet of gas of spec. grav. 0·6, yield the following products:—

	Gas.	Tar.	Ammonia Water.	Coke.	
1	22·25	8·50	9·50	59·75	Average of many experiments.
2	20·01	7·85	7·14	65·00	Manchester.
3	20·40	6·4	5·4	67·85	Dukinfield.
4	21·7	7·5	5·8	65·0	Macclesfield.
5	16·3	10·7	8·0	65·0	Salford.

From a careful series of experiments made by a large tar distiller, the following numbers are derived, showing the average composition of gas tar:—

100 tons of Tar on distillation yield:—

	Naphtha.	Light Oils and Carbolic Acid.	Heavy Oils, Naphthalene, Anthracene.	Pitch.	Water, Gas, and Loss.
1	3·0	1·5	35·0	50·0	10·5
2	3·0	0·8	25·0	60·0	12·2

It is from Benzol, $C_6 H_6$, discovered by Faraday in 1825, that the aniline colours are all of them prepared. The colour-producing power of the coal products are, however, yet far from being exhausted. It is by means of another and hitherto comparatively unknown hydro-carbon, *Anthracene*, $C_{14} H_{10}$, that the newest triumphs of the chemist have been won. This is a substance which in the pure state few chemists (even yet) have seen, and upon which only two or three had previously experimented, and yet by one happy discovery—and by an investigation which more than almost any other exhibits the value of the synthetic power of modern research—this unknown body has been made to yield a colouring matter of the greatest possible value. The truth of this will at once be evident when we learn that the total growth of madder is estimated to reach 47,500 tons per annum, worth 45*l.* per ton, and having, therefore, a value of 2,150,000*l.* Of this nearly one-half is used in the United Kingdom, so that no less a sum than 1,000,000*l.* is now paid by us for madder grown in foreign

countries. This will now, in part at least, go to benefit our own population, as we can now transform our coal into this invaluable colouring matter.

In an experiment made on a large scale it was found that 100 tons of tar yielded 0·63 ton of anthracene, or 1 ton of anthracene can be obtained from the distillation of about 2000 tons of coal, not reckoning the quantity of anthracene contained in the pitch.

Madder is the root of several species of Rubia, amongst which the *Rubia tinctorum* is the most valued for its dyeing properties. This grows in Holland, Asia Minor, and in the south of France and of Russia. A species native to England is the *Rubia peregrina*. This belongs to the order *Rubiaceæ*, the native members of which, as the Galiums, are mostly inconspicuous wild plants. Some of the foreign species are on the contrary important plants, such as the Cinchona, Ipecacuanha, and Coffee plants, and these are distinguished for the number and variety of the peculiar principles which they yield, as quinine, cinchonine, caffeine, alizarine. Thanks to the kindness of Dr. Schunck, the speaker was able to show a young madder plant.

In spite of the many investigations which have been made of madder, chemists are still in doubt as to the nature of many of its constituents. Some attribute its colouring powers to the presence of at least two substances—alizarine and purpurine—whilst others say that only one of these produces the true madder colours.

Alizarine was discovered and obtained from madder, as a crystalline sublimate, by Robiquet and Colin, in 1831, but little importance attached to this discovery until Schunck, in 1848, showed that all the finest madder colours contain only alizarine combined with bases and fatty acids. The second colouring matter, termed *Purpurine*, was discovered by Persoz. It contributes to the full and fiery red colour in ordinary madder dyeing, but dyes a bad purple, alizarine being essential to the latter. Purpurine disappears during the purifying processes of soaping, &c., being far less stable than alizarine. It is distinguished from alizarine by its solubility in boiling alum liquor.

These two colouring principles may likewise be easily distinguished by the spectrum, alizarine producing a set of dark absorption-bands, quite different from those of purpurine, which again vary according to the nature of the solvent. Alizarine can be obtained in yellow needle-shaped crystals by simple sublimation from the dried madder; but this colouring matter is, singularly enough, not contained ready formed in the fresh madder root, but is the product of a peculiar decomposition. For a proof that fresh madder does not contain alizarine we have only to extract the moist root with alcohol, when neither the alcoholic extract nor the insoluble residue will be found to possess tinctorial power. We owe this knowledge to the researches of Schunck and Higgin, who have proved that alizarine is produced by a peculiar kind of fermentation which partly occurs in the root on standing, and partly takes place in the dyebeck, when the powdered madder is treated with water. A crystalline glucoside, termed Rubianic Acid

(Schunck), is contained in the root, and it is this which splits up simply into alizarine and glucose. This acid crystallizes in fine yellow needles, and gives a definite and crystalline potash salt, from which it was shown to contain 26 atoms of carbon in the molecule. Hence, as no other product but glucose is formed, it follows that alizarine must contain $C_{26} - C_{12} = C_{14}$. This decomposition of rubianic acid into alizarine was shown by boiling with an acid, and adding caustic soda when the blue solution of alkaline alizarine was seen. The formation of alizarine in extracts of madder root is effected by a ferment peculiar to the plant, and called Erythrozym. It is a ferment *sui generis*, since no other ferment produces the same effect. When mixed with a solution of rubian or rubianic acid, at the ordinary temperature, the latter is rapidly decomposed as with acids. This is what takes place in making *fleur de garance*. Dyers raise the temperature of their madder-baths gradually up to the boiling-point, because the application of a high temperature destroys the ferment. When the temperature is gradually raised, the ferment acts upon the glucoside, and produces alizarine.

That the colouring matter in fresh madder root is not alizarine can be easily shown by rubbing the soft portions of the root on to paper, when a yellow stain will be produced, which, on treatment with an alkali, shows the bright red colour of an alkaline solution of rubian instead of the blue solution of alizarate.

According to Schunck, the origin of purpurine, and its relation to alizarine, are still involved in obscurity.

The hypothesis which of late years has done more than any other to stimulate experiment and enlarge our views in organic chemistry is undoubtedly Kekulé's theory of the Tetrad nature of Carbon and his explanation of the constitution of the Carbon Compounds. In the so-called Paraffine group of organic substances, the carbon atoms are supposed to be connected together by single links of the four bonds attached to each atom, thus giving rise to saturated compounds by the attachment of other elements or radicals to the free bonds. In the group of aromatic substances with which we are specially concerned the carbon atoms are more closely linked together, or in other words, a less number of atoms of hydrogen are necessary to saturate an aggregation of carbon atoms than is the case in the other group. We can explain this, upon the assumption of the tetrad character of carbon, by supposing that each carbon atom is attached to its neighbour alternately by one and by two bonds.

Another singular property of these aromatic bodies is that they all contain at least six atoms of carbon and that the simplest hydrocarbon of which they are made up is Benzol C_6H_6. So that we may regard all these aromatic compounds as Benzol derivatives, and this hydrocarbon may be considered as the skeleton round which many complicated substances are arranged. So that by the replacement of one atom of hydrogen by NH_2 we obtain Aniline, by OH Phenol, &c. From the knowledge gained by his investigation on the Quinones,

Graebe came to the conclusion that alizarine belongs to the Quinone series; and, availing themselves of Baeyer's reaction, by which Phenol can be converted into its hydrocarbon Benzol, Graebe and Liebermann passed the vapour of natural alizarine obtained from madder over heated zinc-dust, and found that the hydrocarbon they formed was identical in all its properties with Anthracene $C_{14}H_{10}$ from coal tar. Hence they confirmed Schunck's conclusions that the molecule of alizarine contains fourteen atoms of carbon. Having thus got hold of the backbone, as it were, of the compound, it only remained for them to clothe the hydrocarbon with the four additional atoms of oxygen and to take off the two atoms of hydrogen in excess, in order to obtain alizarine.

Laurent and also Anderson had, many years ago, obtained a body of the composition $C_{14}H_8O_2$, and Graebe recognized this as the Quinone of Anthracene; and he now only required to replace in this two atoms of hydrogen by two of hydroxyl (O H), in order to obtain alizarine, which clearly appeared to be a quinone acid—

$C_{14}H_{10}$	$C_{14}H_8(O'')_2$	$C_{14}H_6\begin{cases}O_2''\\OH\\OH\end{cases}$
Anthracene.	Anthraquinone.	Alizarine.

This replacement of hydroxyl can be effected by Bromine, by which Bibromanthraquinone $C_{14}H_6Br_2O_2$ is formed, and this, on fusion with caustic potash, gives potassium alizarate, yielding pure alizarine on treatment with hydrochloric acid. The high price of bromine rendered this process unavailable for manufacturing purposes, and hence another plan was simultaneously proposed by several chemists for effecting the same end in a cheaper mode. Use was hereby made of Kekulé's and Wurtz's reaction in the formation of sulpho-benzolic acid. On treating Anthraquinone with strong sulphuric acid to a high temperature, the di-sulpho acid $C_{14}H_6O_2\begin{cases}SO_3H\\SO_3H\end{cases}$ is formed, and this, on heating with concentrated solution of potash, yields the sulphite and alizarate of potassium; from the latter substance pure alizarine is obtained by the action of acids.

In the following Table we have a statement of the synthetic production of alizarine from its constituent elements:—

Synthesis of Alizarine.

1. Acetylene by direct union of Carbon and Hydrogen in Electric Arc.
   $C_2 + H_2 = C_2H_2$ (Berthelot, 1862.)
2. Benzol (Tri-acetylene) from Acetylene by Heat.
   $3\,C_2H_2 = C_6H_6$ (Berthelot, 1866.)
3. Anthracene from Benzol and Ethylene.
   $2\,C_6H_6 + C_2H_4 = C_{14}H_{10} + 3\,H_2$ (Berthelot, 1866.)
4. Alizarine from Anthracene. (Process No. 1.)
   (Graebe and Liebermann, 1869.)

   (A) Oxyanthracene or Anthraquinone by Nitric Acid.
   $C_{14}H_6(OH)_2$ (Anderson, 1861.)

SYNTHESIS OF ALIZARINE—*continued.*

(B) Bibromanthraquinone by action of Bromine.

$$C_{14}H_8O_2 + 2Br_2 = C_{14}H_6Br_2O_2 + 2BrH$$

(C) Alizarine by action of Caustic Potash.

$$C_{14}H_6Br_2O_2 + 4KHO = C_{14}H_6(OK)_2O_2 + 2KBr + 2H_2O$$

Potassium alizarate.

5. Alizarine from Anthracene. (Process No. 2.)
(Graebe and Caro, Perkin, Schorlemmer and Dale.)

(A) Disulphoanthraquinonic Acid from Anthraquinone.

$$C_{14}H_6(OH)_2 + 2H_2SO_4 = C_{14}H_6O_2\left\{\begin{matrix}SO_3H\\SO_3H\end{matrix}\right\} + 2H_2O$$

(B) Alizarine from the above by the action of Potash.

$$C_{14}H_6O_2\left\{\begin{matrix}SO_3H\\SO_3H\end{matrix}\right\} + 4KHO = C_{14}H_6O_2\left\{\begin{matrix}OH\\OH\end{matrix}\right\} + 2K_2SO_3 + 2H_2O$$

Alizarine.

Mr. Perkin states than an intermediate substance is formed in this reaction having the formula $C_{14}H_6(O)_2''\left\{\begin{matrix}OK\\OSO_3\end{matrix}\right.$, and this, when heated with potash, splits up into alizarine and a sulphite. Other yellow-coloured products are, according to Perkin, contained in the alizarine as sent out from his manufactory. The nature of these yellow crystalline bodies is as yet unknown.

Of the identity of the natural with the artificial alizarine there can be no doubt: they agree in all their physical and chemical properties. Their absorption-spectra are identical, their tinctorial powers are the same; the coloured lakes which they form with alumina, iron, and copper salts are of the same tint and possess the same degree of solubility, and these remain alike unaltered by the action of light, so that when they are fixed in the cotton-fibre they yield equally fast colours.

It is difficult to predict how far the artificial alizarine will in future restrict the growth of madder; but there is no doubt that for many styles of calico-printing the artificial alizarine is of the greatest value, and we may naturally expect to see very important changes effected in this branch of chemical industry in the further practical application of this new discovery.

CONTRIBUTIONS TO THE HISTORY OF ALIZARINE. $C_{14}H_8O_4$.

1825. Faraday discovered Benzol in Coal-gas Oil. C_6H_6.
1831. Robiquet and Colin discovered Alizarine in Madder Root.
1832. Dumas and Laurent discovered Anthracene in Coal Oils.
1848. Schunck gave the Composition of Alizarine. $C_{14}H_{10}O_4$.
1850. Strecker " " " $C_{10}H_6O_2$.
1862. Anderson examined Anthracene Compounds. $C_{14}H_{10}$.
1865. Kekulé explained the constitution of the Aromatic Compounds.
1866. Baeyer obtained Benzol from Phenol.
1868. Graebe investigated the Quinones.
1868. Graebe and Liebermann obtained Anthracene from Alizarine.
1869. " " " Alizarine from Anthracene.

[H. E. R.]

GENERAL MONTHLY MEETING,

Monday, April 4, 1870.

Sir HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Fredk. Braby, Esq. F.G.S. F.C.S.
Bernard Cracroft, Esq.
Henry Doulton, Esq.
William Evans, Esq.
Frederick Geo. Wm. Fearon, Esq.
Lieut.-General Frederick William Hamilton, C.B.
John Fowler, Esq.
Mrs. Ellen Margaret Joachim.
Thomas Hawksley, M.D. M.R.C.P.
W. G. Lettsom, Esq.
Miss Anne Egerton Smith.
Sebastian Smith, Esq.
Charles Sweet, Esq.

were *elected* Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Ansell, G. F. Esq. (the Author)—The Royal Mint. 8vo. 1870.
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WEEKLY EVENING MEETING,

Friday, April 8, 1870.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Professor Huxley, F.R.S.

On the Pedigree of the Horse.

[Abstract deferred.]

WEEKLY EVENING MEETING,

Friday, April 29, 1870.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Professor J. S. Blackie, F.R.S.E.

On Scientific Method in the Interpretation of Popular Myths, with special reference to Greek Mythology.

There is perhaps no subject of great known interest and deep philosophical significance which has been so much neglected by British scholars as Mythology. We have scarcely half-a-dozen books to show on the subject; and of these the most notable and the most original, that of Bryant, found on the shelves of most good English libraries, is, with all its merits, after all only a grand chase in the dark, with a few bright flashes of discovery and happy gleams of suggestion by the way. Payne Knight's treatise on Ancient Symbolism contains many just and admirable observations; but it does not pretend to be a systematic treatise on Hellenic mythology. Recently the subject has been taken up by the subtle and versatile intellect of Mr. Gladstone; and German notions have been poured in upon us wholesale under the masterly manipulation of Max Müller. But the one-sidedness and inadequacy of most of these recent attempts plainly indicate that we are as yet only in the first stage of a really scientific treatment of myths, and that a considerable amount of fervid speculation on the subject will require to have spent its young force before the foundations of any stable doctrine on the subject shall be laid. The fact seems to be that the English mind, so great in practical matters, in certain interesting fields of speculation presents

almost a total blank; and when at length questions of deep human significance belonging to these fields are raised, our lethargic intellect, awakened out of a long slumber, unable at first to discern between truth and falsehood in a strange element, is dazzled and stimulated into a transcendental admiration of the first new idea, through the instrumentality of which it has found the delight of a new consciousness. In these circumstances it may not be without profit to take a cool survey of the ground; and, if nothing better, at least to furnish young adventurers in this field with a few cautions that may save them from repeating in another form the adventurous Quixotism and brilliant blunders of Jacob Bryant.

For the sake of distinctness, I will state what I have to say on this subject in a series of propositions.

I. The Greek mythology in its main points and prominent features was the product of a vivid imagination and a subtle fancy, acted upon and reacting on the great forms and forces of the external world and the mysterious Powers of the inner world in such a manner as, according to the natural laws of imaginative activity, to create a beautiful procession of divine personages and divine actions, framed after the similitude of human persons and human actions. In other words, Greek mythology is a religion and a poetry of nature, or a highly poetical anthropomorphic physico-theology.

This proposition I state as one which may be called broadly the general result of the researches of the distinguished band of mythological students in Germany, from Heyne to Welcker, Hartung, and Max Müller, whose erudite labours have so cast into the shade the feeble glimmerings of light that could be gathered from our somewhat narrow and arid school of native scholarship. It is a proposition resting on a wide induction, and applicable not to Greek mythology only, but, in a greater or less degree, to the mythology of all peoples who have not sunk into a state of utterly gross and embruted existence. It is a proposition also from which Mr. Gladstone, who has a special theory as to the origin of certain of the persons in the Greek Pantheon, will not withhold his general assent. Let us see, therefore, what elements, according to this view, the theological myths of the Greeks ought to contain. Now here it is plain that those objects of external nature which are most striking in their presentation and most potent in their influences, would in the first place be subjected to the transmuting action of a vivid and reverential fancy in the early races of mankind; and these objects, of course, are the heavens, the earth, the sun, the moon, the stars, the ocean, the rivers, the generative and procreative virtue, and the underground world with the powers of darkness and of death. But man is not, even in his lowest stage (whatever the French Helvetius and certain of his English disciples may conceit themselves), a creature of mere sensations, made up wholly, to use Hume's language, of impressions from without. In his lowest stage, he has passions, feelings, sentiments, ideas, and thoughts, of which, though the occasion be sensuous and external, the real spring is

mental and internal; and accordingly, unless we suppose him by some awful calamity cut short in his rational development and reduced to a mere animal, his imagination, acting upon the invisible Powers of the world within, will create impersonations of these powers, after a fashion similar to the anthropomorphic transformations produced by the same magic touch in the external world. The only difference here may likely be, as in the formation of language, that, instead of creating different gods to represent the inward mysteries, the mind, which always works by analogies, will superadd a mental significance to some impersonated power of the external world previously formed, and thus recognize the outward and the inward as fundamentally one. This process, however, does not seem absolutely necessary, as it is quite possible that powerful passions, and visible aspects of passion—such as PAVOR and PALLOR, in Livy (I. 27)—may assume directly the anthropomorphic attitude, without any external inoculation. In this view, I cannot but regard it as a mistaken view of Max Müller to presume as a matter of course that Erinnys, or the Fury, must be identified with some goddess of the external world. The account of Pausanias (VIII., 25, 6), according to which this terrible impersonation of conscience, or the violated moral law, is derived from ἐρινύειν, an old Greek verb originally signifying *to be angry*, has sufficient probability, not to mention the obvious analogy of Ἀραί, another name sometimes given to the awful Maids (σεμναί), from ἀρά, an imprecation (Æschyl., 'Eumen.').

II. In order to discover what particular form, force, or power of Nature, or the inner man, is represented by any figure in a mythological procession, the first thing to be done is to bring the special figure with all its symbolical accompaniments, and in all its traditional completeness of aspect and attitude face to face with Nature; and then proceed, in obedience to the natural action of a reverential and poetical imagination, to identify the sign with the thing signified, and cause the god anew to leap as it were directly out of nature, and assume a human shape before a human imagination.

Those who wish to see how this may be done, may consult the well-known passage descriptive of the origin of Greek mythology in the fourth book of Wordsworth's "Excursion." Here the poet of the mountains, the woods, and the floods, sets himself to re-create the figures of the Greek gods from the living environment of woods, mountains, and floods out of which they arose; and there can be no doubt that this process of re-creation of a lost nature-worship is scientifically legitimate. "*Omnis scriptura sacra*," says Thomas à Kempis, "*eo spiritu legi debet quo scripta est;*" and in the same way the signature of a religion of nature in an anthropomorphic theology can be understood only by a mind contemplating nature under the same conditions and influences that were present when the theology was formed. It is only, therefore, from the combined action of poetry, piety, and philosophy—such as existed in the soul of Wordsworth—that a recovery of the significance of the symbolic representations of a religion of

nature is to be looked for; mere science will not suffice, and the most extensive and curious erudition can only supply the materials of the reconstruction, not assist in the architecture. These materials are of the utmost consequence; otherwise (like Ruskin*) we shall be amusing ourselves, building an imaginary palace in the air for the habitation of an imaginary queen of the air. They must be collected also with great care and labour from many curious and remote sources; for we must not imagine that any secular poet—even Homer, much less such a luxuriant fancy-monger as Ovid—can supply us on all occasions with the true character and significance of some of the most important personages in the motley Greek Pantheon. The Greek religion, like the Greek people, was tribal and local to a degree which we moderns, with our one Catholic Church, may find it difficult to realize; but a few very superficial glances into the pages of Pausanias, or a slight acquaintance with the Vase Chamber of the British Museum, will teach the mythologic student how little Homer and Hesiod, notwithstanding the eulogy of Herodotus (II. 53), can be looked on as the complete Bible of early Hellenic theology. Extensive and well-sifted Greek learning will, therefore, on all occasions be necessary before a successful solution of any of the more difficult problems of Greek mythology can be attempted; and of this sort of learning the works of Ottfried Müller, Welcker, and other distinguished German writers, furnish us with abundant stores, if we only had the chaste imagination and the sober sense to make a good use of them.

III. In determining the character of a mythological personage reference may always be had to the etymology of the name, and the significance of the epithets; but this is an instrument which a wise man will employ with the most jealous caution, and always as an adminicle of evidence only, never as the foundation of an argument.

It is needless for those who are in the slightest degree acquainted with works on mythology, to recall what wild work has been habitually played with this potent wand of learned harlequins. In the hands of a Bryant of the last century, or an Inman of the present, etymology, as a manipulator of myths, is no wise interpreter, much less an infallible oracle, but only a clever juggler. A Celt, it has been said, whenever he meddles with etymology, becomes mad; and this witness, I am sorry to say, is in no small degree true; but even under the scientific guidance of a Bopp, a Pott, a Grimm, and a Müller, a sober man may sometimes even in the full blaze of the new Sun of comparative philology, allow himself to drink deep draughts, if not of maundering madness, at least of manifest hallucination. For in the hot chase after facts to illustrate a favourite theory learned men do not always sufficiently consider that the original form of these venerable old names (without which etymology is mere guessing) may be lost, and is not now recoverable; further, that two or three different interpreta-

* 'The Queen of the Air,' by John Ruskin. 1869.

tions, all according to philological laws quite legitimate, may be equally plausible; and, if the etymology is sought for, not in the native tongue, but in some foreign dialect, such as Hebrew, Sanscrit, or Coptic, the danger of slippery transmutations and conjectural illusion is of course increased tenfold.

IV. The general statement in the first proposition leaves the question still open, whether, granting the principal personages in the Hellenic Pantheon are mere impersonated elements of Nature, there may not also be found in the wide and various range of mythological tradition some decidedly historical element. The answer to this question plainly is, that such an element is by all means to be presumed in all collections of popular traditions; and the burden of proof, as the lawyers say, lies clearly with whosoever asserts the contrary.

In reference to this point, the cool way in which Max Müller and his English disciple, Mr. Cox, assume that there are no human figures and historical characters in the whole gallery of heroes and demigods in the Greek mythology, is something very remarkable. The idea that any people which, in its earliest history, possessed an age of great men and heroes, should systematically blot out the memory of all this very substantial company, and substitute in their place a floating conglomerate of mere crystallized epithets of forgotten and humanized types of degraded gods, is so manifestly improbable, that one is set seriously to fish out some secret and concealed cause of so startling an effect. Nor is the cause far to seek. This extravagant application of the doctrine of Euhemerus—whose heresy consisted only in his assigning a human origin to Jove and the other great gods—is to be traced to a peculiar idiosyncrasy of the German people, who, from the earliest germ of their great scholarship, have shown an instinctive aversion to external facts, and an extravagant passion for self-evolved ideas. To suppose that Achilles was merely what he appears to be, a Thessalian captain, to the German mind appears a prosaic proposition, altogether beneath the notice of the eagle-flight of their transcendental imagination. He must be a water-god, or a sun-god, or the human representative of whatever theological idea the necessities of a despotic system may require. In England such an aversion to the recognition of fact in tradition is quite a recent phenomenon, and takes its rise, not from a ripe judgment and a sober conviction, but merely as an extreme reaction against the old historical school of Clinton, and the stout English realists of the last century. In Porson's day it was the fashion to laugh at the Germans, their philosophies, their principles, their ideas, their symbols, and even their doctrine of metres; but now we have got a new light, and with the known zeal of green converts, we are ready to receive passively and swear to the mere nonsense of those very men, whose sense, only a few years ago, we were too ignorant and too insolent to recognize. This state of affairs in one section of our intellectual world is certainly not one of which John Bull has reason to be proud. We find him

from the extreme worship of the letter, and a faith based on written authority, to a wild chase after symbols, an easy reception of all foreign fancies, and a wholesale historical scepticism, as far removed from a sound historical judgment as the wholesale credulity which it has supplanted. It will require another generation to brush away the cobwebs which this German insanity is teaching us so luxuriantly to spin.

V. The theory of the Germans, of which a recent English mythological writer has become passionately enamoured, is directly the reverse of the old theory of the sceptical Messenian. Euhemerus taught that all the gods were originally deified men; the Germans and Mr. Cox teach that all the men were originally degraded gods. This theory, like the other, has its partial truth and its proper place. The mistake, or, as we should rather say, the madness, consists in its universal and indiscriminate application.

A slight consideration may teach us how a god, or a mere epithet of a god, may come to be believed in as a mere man. The human aspect and attitude which it is the essence of anthropomorphic theology to assume, must often render it difficult to know whether any particular narrative describes the actions of a god or of a man. This difficulty, lying in the mere form of the tradition, vanishes, of course, to the native believer, from his previous training, and consequent recognition of the real character of the god. But suppose a strange element, by conquest or otherwise, to enter into the population. The stranger has no previous knowledge of the significance of the divine histories, and so may very naturally take them for something purely human; just in the same way as a race of conquerors settling in a country is often found in the most perverse and ludicrous way to misinterpret and misapply the topographical names of the places where they make their settlement. An obvious and generally recognized example of this we find in the Syrian Adonis, who, to the framers of the myth, no doubt was a mere impersonated symbol of a physical phenomenon, to a devout polytheistic fancy sufficiently intelligible; but who, in the hands of the Western Greeks, when they got hold of him, became only the dainty hero of an erotic idyll, and a type of such juvenile beauty as a sensuous Aphrodite might admire.

VI. Though in the early stages of human thought it is an extremely difficult thing to distinguish physical science from theology, we may nevertheless recognize a special class of myths which for the sake of distinction may be called scientific myths. These are myths that seem to represent in a personal and narrative form certain natural phenomena, at the present day familiarly known under certain categories of natural science.

What this proposition points to may be best illustrated by what Pausanias (Attica 21) says about the Niobe of Asia Minor. That patient and painstaking topographer relates that when he was passing through Phrygia, in the vicinity of Sipylus, he saw a fantastic rock, which, viewed from certain points, had the appearance of a weeping

woman. Now, what I wish to indicate here is, that certain mythological legends may have originated from such geological phenomena; either in the way of imaginative manufacture, pure and simple, or at least in the way of connecting such physical appearances with traditional historical persons. Those who have travelled in the Hartz mountains will perhaps recall certain legends of this nature, evidently invented by the popular fancy for the purpose of explaining how Nature happened to put on such grotesque shapes, and assume an attitude so fantastic. In the same way the legend of the fire-breathing Chimæra will most readily be referred to the fountains of flame which travellers have recognized as issuing from diverse parts of that volcanic region; and the mythological stories of the giants, those masterful rebels against Jove's high sovereignty, are in not a few cases connected with regions where volcanic action upon a large scale even now forces itself on the most unspeculative eye. So absurd is it to suppose that any one theory, however plausible, has an exclusive right to be legitimated by the stamp of science as the only rightful interpreter of the strange carnival of forms which the busy creativeness of the popular imagination may in any country have put forth.

VII. After all native sources have been exhausted—that is, when the language and the literature, and the artistic remains of any people have been thoroughly sifted, and failed, when placed front to front with Nature, to give a key to the significance of any mythological figure—then, and not till then, will it be wise to refer to foreign sources of information; and the comparative value of these foreign sources will depend either upon original connection of races, or upon accidental historical inoculation, or upon both. The problem of the mythologist will then be to trace out by a careful comparison, and, if it may be, direct historical deduction, the original identity of any two or more gods worshipped in some most distant regions of the earth. For the practical application of this proposition the instinct of mythological inquirers was led at an early period, so far as Greece was concerned, to recur to Egypt. The key-note here was struck by Herodotus (II. 50), to which in modern times Bryant and Blackwell showed a forward willingness to respond; but the researches of modern Egyptologists have not enabled us to plant upon any scientific foundation the conjectures of the ancients with regard to the influence of the land of Ham on the early religion of Greece. It rather appears, indeed, that the likeness pointed out between the personages of the Egyptian and Hellenic Pantheon are of that superficial and altogether deceptive character, in which polytheism, from a natural instinct of fraternization, always delighted to indulge; and, historically, there is no connection either of original race or of later communication which should justify us in presuming that any portion of the earliest Greek theology is derived directly from the Coptic Delta. Next to Egypt, Palestine was for a long time the favourite source to which the Hellenic theology used to be traced; our old scholars and divines, Vossius, Bochart, Gale, and others were of this

opinion. Some aspects of it have been put forward recently in an original way by Mr. Gladstone; but the more this theory is examined the less it appears tenable; and the pious Christian prepossessions—originating with the Church fathers on which it was founded—melt into nothing at the touch of an impartial and discriminating criticism. For, in the first place, it is evident that if the Greek myths contain any element that may fitly be derived from a primitive sacred tradition held by the Hebrew patriarchs, that element must be the unity of the Deity contained in the person of Zeus as supreme moral governor; but this is precisely the idea which gives no hint towards explaining that motley variety of figures in which polytheism properly consists; and besides, one may say that some vague idea of the unity of God is so necessary towards any consistent structure of celestial government, that it could not but rise up spontaneously in the minds of a thinking people like the Greeks, without the necessity of referring to any external source. Then as to the special identification of certain polytheistic personages—Athena and Apollo—with the persons of the Christian Trinity which Mr. Gladstone has delighted to make, one can only say that the marks of identity seem too broadly human and general to admit of being confined to any such particular origin. The figures of a wise goddess and a bright god, the Purifier, might exist in any form of polytheism, springing, as they naturally do, from the feelings of the human heart stirred in the creation of an anthropomorphic system. Any further use of the Hebrew traditions, such as has been made in the identification of Dionysus with Noah or other patriarch, is justifiable only on an application of the Euhemeristic principle far beyond the natural and rational limits to which, in the opinion of the present writer, it ought to be confined. After Palestine, the neighbouring country of Phœnicia comes in as a claimant for the high honour of having given birth to certain of the Hellenic gods. This claim also has been put forward by Mr. Gladstone with more than his usual subtlety and ingenuity; but, while the general point may be conceded that the long-continued influence of the Phœnicians on the coasts of the Mediterranean could not remain without leaving some traces of mythological mixture and fusion, such as may be traced clearly in the figures of Hercules and Aphrodite, it does not appear safe to launch largely into this region with the hope of making any brilliant discoveries. Poseidon, in particular, amid a maritime people like the Greeks, may be safely held to have sprung from his native Greek waters, unthoughtful of any more lofty genealogy from Tyre.

But it is to the Indus and the Ganges that the most recent speculators have taught us to look for the true word that shall solve the riddle of the Sphinx of Hellenic mythology; and it must be confessed that the prospect in this quarter, at first blush, looks considerably more hopeful. For in this case not only have we a striking identity of race; but the fancy is at once flushed with the conception, that the same brilliant discoveries may burst upon us from comparative

mythology, as have already shone upon the philological world in full meridian glory, from the discovery of the identity of the Greek and Sanscrit languages. May it not be that father Jove, and mother Cybele, and daughter Diana, have made the long journey from Mount Meru to Mount Olympus, with as little trouble or transmutation as the Sanscrit words corresponding to *father*, *mother*, and *daughter*, have experienced in travelling from the Indus to the Ilissus, and from the Ilissus to the Cuchullin hills in Skye? But in this first thrill of bright hope there start up certain plain considerations calculated in no small degree to damp the ardour of a sober inquirer. The peculiar difficulty in the case of comparative mythology lies plainly in the shifting and mutable character of the materials from which the comparison must be made. Contrast a theological myth, in this view, with a geological stratum. Myriads, or millions of ages ago, let us say, large sheets of certain earthy deposits were laid down in the north-west of Scotland, in Assynt, and about Cape Wrath. Once there, they are there for ever, safely buried beneath the cover of superior strata. In such a position no harm can reach them, no change can touch them. If then, in the far West, on the banks of the St. Lawrence, or in the extreme East, similar deposits shall be found to exist, protected in a similar way from the influence of superficial change, it is plain that a comparison made between these strata proceeds from data in a sense as fixed and determinate as those which limit the deductions of the mathematician. No disturbing influence has interfered; nothing certainly that falls under the category of transmutation can have taken place. Take again philology. Words are no doubt much more liable to curtailment and contraction, to distortion, and disguise of all sorts, than rocks. It requires a practised scientific eye, for instance, to recognize the verb ὄπτομαι, in the modern Greek μάτι (for ὀμμάτιον), *an eye*; and language is full of such obliterations. But a very little historical knowledge will enable a classical scholar to trace back with scientific certainty the disguised form of Aryan words in Roman and the Romanesque languages, to their original types in ancient Greek and Latin. There is a stable and indeed a very obstinate element in language, which remains fixed amidst all the confusion which centuries of social disorder and intellectual disrepair may have introduced. With mythology, on the other hand, from the very nature of the case, this fixed element, which is to form the basis of a science of comparative mythology, is reduced to a minimum. The same fancy which created an Indra beneath Indian skies, will be creating a Jove beneath the blue of a European Olympus; or, if not absolutely creating, will employ itself so constantly in the process of modification, that the points of similitude in the end will bear a very small proportion to the points of difference. To borrow a phrase from Max Müller himself, the myth-forming imagination is "a kaleidoscope" of constantly shifting figures; nay, more, it is often a harlequin's wand, of the results of whose magic touch no rational account can be given. Ancient faiths

were not, like modern creeds, stereotyped in Articles, so as through the long course of hundreds and thousands of years to defy change and to challenge innovation. Innovation rather was the law of their existence, and the spectator was pleased with the floating beauty of a cloud which began as a whale and ended as a lion, or a bear, or any imaginable counterfeit. Under such circumstances one cannot but stand aghast at the assurance with which Mr. Cox, for instance (reflecting here as elsewhere the German views of Max Müller), asserts that the Iliad of Homer represents a grand physical struggle between the powers of Light and Darkness, in which Achilles is the champion of the good principle, and Paris a Trojan Ahriman of evil. But Mr. Cox himself, in the explication of the allegory, is forced to admit that to the eye of Trojan patriotism, this prince of darkness necessarily assumed the character of a prince of light; and notwithstanding all this transmutation he most potently believes that the original darkness out of which the figure of the light-god Paris was shaped, is still distinctly traceable through the bright disguise which he has assumed! A man must certainly feel extremely satisfied with his own perspicacity who can see through every Protean form of mythological metamorphosis in this way; but he must not expect that common mortals shall be able to look with unwinking vision on such a dazzling legerdemain of the great juggler Fancy. Mr. Cox's method of mythological investigation, indeed, is from beginning to end unscientific. He starts with the bright prepossession that whatever gods are worshipped on the banks of the Ganges must be found also on the banks of the Eurotas; and where points of diversity are systematically disregarded, it requires only a little Teutonic ingenuity to stilt up a few points, it may be of mere superficial and accidental similitude, into the formal dignity of a proof. If Achilles takes the sulks and leaves the Greek camp for a season, and if Meleager, in the war between the Calydonians and the Curetes, does the same; what can this mean but that they are both sun-gods, and that the glorious luminary, for a day and an hour, has hid his gracious radiance under a storm-cloud! with such a style of reasoning anything can be made out of anything. It is in fact not a whit more rational than the most fanciful imaginations of Bryant, differing only in this, that whereas in those days it suited the fashion of the hour, or the whim of the writer, through strange Egyptian disguises to recognize Greek gods, at the present hour Indra and Agni, and Krishna, are preferred as the great originals of the bright Olympian conclave. But nothing can stand as science which is based on the intoxication of a favourite fancy; the dreams of Bryant, notwithstanding his curious erudition and marvellous ingenuity, have long ago passed away as mere dreams; the shelves of German libraries also are groaning with volumes of mythological lucubrations, that even now, in the short hour of their life, are generally registered as devout imaginings. The Sanscrit novelty in like manner will have its day; and after the first dazzlement shall have passed away, men will rub their eyes and

ask themselves how it was possible that they should ever have allowed themselves to be fooled by such hallucinations.

There is one point of view from which Mr. Cox looks on the Greek mythology that has not yet been mentioned. He feels himself thrilled with a moral horror at the idea of cultivated people like the Greeks having ever been in such a low moral state as to imagine such monstrous tales of adultery, incest, and murder, as true histories about their gods and heroes. These monstrous tales are therefore in all cases to be received as misunderstood theological myths. Now there can be no doubt that the amours of Zeus, for instance, are not to be taken literally as gratifications of mere sensual passion. They represent unquestionably some physical interaction (like the sacred marriage of Heaven and Earth in Iliad, XIV.); or we may say that a sort of general fatherhood of Jove and a polygamic connection with mortal women was necessary to fill the earth with heroes and demigods. "*Sunt superis sua jura!*" Polygamy in the great Father of the Universe may be a benignant semination of divine gifts and graces, which in the head of a human family could lead only to confusion and the vague concubinage of savage life. So far well. But when it is argued further that because the old family history of the Labdacidan and Pelopidan kings, as represented in the Greek drama, contains monstrosities and atrocities which could not possibly have existed in connection with any real human characters among such a people as the Greeks, it is forgotten not only that the history of great despotic and dynastic houses has seldom been free from the taint of great crimes — as the story of the decadent Roman autocracy, of the Byzantine Empire, of the Popes of the fifteenth century, not to mention the kings of Judah, including the well-known King David himself—but it is kept out of view also that when horrid crimes are committed, the popular imagination experiences a morbid delight in exaggerating their horrors; and, to bring the thing to a climax, if a generation of tragic poets or sensation novel writers are at hand, there is no limit to the intensity of sanguineous and pitchy hues with which such real occurrences may be painted out in the popular imagination. Not therefore because a Theban dynast in the local tradition is said to have committed incest unknowingly with his mother, nor because a famous Attic prince is believed to have carried off Spartan Helen, and even to have descended into Hades, does there arise the slightest presumption that the original type of these traditional heroes was a Sanscrit sun-god. If marvellous lies are told of modern Englishmen, why not of ancient Greeks?

VIII. It remains only that the principles laid down in the above propositions should be tested by their application to some of the leading and best-known figures in the Greek Pantheon. A few sentences will be sufficient for this, as ample indication has been given already of the method of determining the signification of some of the principal personages. Let us select from the mass Zeus, Poseidon, Apollo, Artemis, Hera, Athena, Hermes, Dionysus, Aphrodite, Æscu-

lapius, Hercules, Bellerophon, Achilles, Theseus. Zeus and Poseidon explain themselves obviously enough, the one as the dark-thundering electric energy of the sky; the other as the broad-breasted, wide-girdling, strong-scourging energy of the sea. The omnipotent ruler of the sky represents also the wisdom that rules in the macrocosm of the great universe, as in the microcosm of the soul of man. Jove in fact, as Supreme Power and Providence, is to the universe exactly what Authority and Polity and Law are to human societies. Apollo, as the Sun, requires no commentary, as little his sister Artemis as the Moon. Wild beasts and hunters go forth in the night to seek for their prey, or snare their game, beneath the glimpses of the moon; Diana, therefore, is the protector of the one and the patron of the other. The night season also is favourable to necromancy and incantation, therefore Hecate (ἑκάτη—the moon that sends her rays from far) presides over witchcraft and necromancy, and becomes at last in her latest modern disguise, a witch herself, and the president of all unholy convocations of extravagant spirits and ill-favoured hags that delight in mischief. Hera, as the wife of Zeus, who is the anthropomorphic Heavens, is the anthropomorphic Earth; a great elemental power, represented also in its productive capacity by Ceres, and by the great Diana of the Ephesians, and the Cybele or mighty mother of the Phrygians. Athena is merely the womanly aspect of Jove, therefore the only-begotten daughter of the Thunderer, the inheritor of his wisdom, the representative of his power, and the only goddess who may at times, without offence, wield his deathful thunderbolt. In accordance with this obvious view of her connection with the dark-clouded lord of the upper regions, the epithet of "flashing-eyed" (γλαυκῶπις), so familiar to us from Homer, must have signified originally the brightness of the clear blue sky that peeps out between the rents of the thunder-cloud, or possibly also the vivid flash of the lightning. This, if I remember rightly, is Welcker's view of the nature of this goddess. The original nature of Hermes is clearly revealed by the phallic symbol which he shows (Herod., II. 51), and the fact that in Homer he appears as a pastoral god of increase (Il., XIV. 491), from which origin, when the day of merchants arrived, he became a god of gain generally, and even a patron of thieves. His other functions and attributes can readily be traced back either to the arts of early shepherd life, of which he was the patron, or to the dexterities which belong to mercantile negotiation. Max Müller's attempted reduction of this god to the breeze which accompanies the dawn, shows to what shifts a man of genius may be driven, when he allows himself to revel freely in the intoxicating element of a favourite idea. As for Dionysus, that he is fundamentally the same as Hermes the phallic symbol and potent viny fervour sufficiently indicate; the legends about him likewise point plainly to a migration of his worship from the orgiastic East to the more decent and sober West. In regard to Aphrodite, her Phœnician origin is universally acknowledged; she follows the Phœnician trading stations from isle to isle,

and may be acknowledged in Corinth and the Piræus, not, however, necessarily without the admixture of some original incarnation of sensual love with which she may have coalesced. Æsculapius is an undoubted example of the partial truth of the doctrine of Euhemerus. In Homer he is plainly a mortal physician, a native of Thessaly, the mother-country of medicine and witchcraft to the Greeks. Hercules also in Homer has the aspect of a simple Hellenic Samson, a man of marvellous bodily strength, very natural to come to light in those wild times—a man by whom, no doubt, many very wonderful feats were achieved, and about whom many more wonderful were believed. The Hercules, however, of the full-blown Hellenic mythology is evidently a much more complex person—an agglomerate, perhaps, of many persons—a mixture certainly of a Theban or Argive Samson with the well-known Melcarth, the sun-god of the seafaring merchants of ancient Tyre, and more ancient Sidon. In this view, that the twelve labours of the son of Semele are the twelve months of the year, and the twelve signs of the Zodiac, is in the highest degree probable. As for Bellerophon, Achilles, and Theseus, enough has been said above to indicate that not the slightest reason exists why we should believe that the first was other than a Corinthian prince, as he appears to be; or why the second should be either a sun-god or a water-god (the favourite German idea before Max Müller), rather than a simple Thessalian thane, like the Douglas or Percy in our Border history; or why, again, the third should not have been as real in ancient Attica as Robert Bruce was in modern Scotland, or Charlemagne in the tradition of the Frankish kings. If Ariosto delighted to revel in beautiful nonsense, this is no reason why we should believe that the popular imagination of mediæval Europe should have delighted to disport itself in a limbo of utter vacuity, and created a great European emperor out of nothing.

The present remarks are not intended to treat with any studied disrespect either the science of comparative mythology generally, or the special interpretations of certain individual Greek myths, for which we are indebted to the learning, genius, and eloquence of Professor Max Müller. My protest is intended only against the sweeping and indiscriminate fashion in which he and his followers, neglecting near and native sources, violently, as it appears to me, force Oriental ideas upon the whole body of an important class of myths which grew and flourished on European ground. I object also to the attempt to give a scientific interpretation of all myths. "*Haud scire fas est omnia.*" Some mythological fables were in their origin too trifling to deserve interpretation; others are certainly too tangled to admit of it: and in this region, as in the more important domain of metaphysics and theology, the wisdom of Socrates and the maxim of Sir William Hamilton will often be applicable: "A contented ignorance is better than a presumptuous knowledge."

[J. S. B.]

ANNUAL MEETING,

Monday, May 2, 1870.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1869 was read and adopted.

The Books and Pamphlets presented in 1869 amounted to 255 volumes, making, with those purchased by the Managers, a total of 388 volumes added to the Library in the year, exclusive of periodicals.

Forty-seven new Members were elected in 1869.

Sixty-three Lectures and Nineteen Evening Discourses were delivered during the year 1869.

Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers and Visitors, and to the Professors, for their services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:—

PRESIDENT—Sir Henry Holland, Bart. M.D. D.C.L. F.R.S.
TREASURER—William Spottiswoode, Esq. M.A. F.R.S.
SECRETARY—Henry Bence Jones, M.A. M.D. F.R.S.

MANAGERS.

John J. Bigsby, M.D. F.R.S. F.G.S.
William Bowman, Esq. F.R.C.S. F.R.S.
Charles Brooke, Esq. M.A. F.R.S.
George Busk, Esq. F.R.C.S. F.R.S.
Warren De la Rue, Esq. Ph.D. F.R.S.
The Earl of Derby, M.A. LL.D. F.R.S.
John Peter Gassiot, Esq. F.R.S.
John Hall Gladstone, Esq. Ph.D. F.R.S.
Sir John Lubbock, Bart. M.P. F.R.S. F.G.S.
George Macilwain, Esq. F.R.C.S.
Sir Roderick I. Murchison, Bart. K.C.B. D.C.L. F.R.S.
William Pole, Esq. M.A. F.R.S.
Lieut.-Gen. Sir Edward Sabine, R.A. K.C.B. Pres. R.S.
The Marquis of Salisbury.
Sir Charles Wheatstone, D.C.L. F.R.S.

VISITORS.

William Ernst Browning, Esq.
John Charles Burgoyne, Esq.
William Dell, Esq.
Thomas Williams Helps, Esq. M.A.
Thomas Hyde Hills, Esq.
Alfred Latham, Esq.
Thomas Lee, Esq.
William Martin, Esq.
Edward Henry Moscrop, Esq.
Rev. Cyril W. Page, M.A.
Edmund Pepys, Esq.
The Lord Josceline W. Percy.
Basil Woodd Smith, Esq.
Thomas Spencer Wells, Esq.
Michael Wills, Esq.

WEEKLY EVENING MEETING,

Friday, May 6, 1870.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Richard A. Proctor, B.A. F.R.A.S.

On Star-grouping, Star-drift, and Star-mist.

Nearly a century has passed since the greatest astronomer the world has ever known,—the Newton of observational astronomy, as he has justly been called by Arago,—conceived the daring thought that he would gauge the celestial depths. And because in his day, as indeed in our own, very little was certainly known respecting the distribution of the stars, he was forced to found his researches upon a guess. He supposed that the stars, not only those visible to the naked eye, but all that are seen in the most powerful telescopes, are suns, distributed with a certain general uniformity throughout space. It is my purpose to attempt to prove that—as Sir Wm. Herschel was himself led to suspect during the progress of his researches—this guess was a mistaken one; that but a small proportion of the stars can be regarded as real suns; and that in place of the uniformity of distribution conceived by Sir Wm. Herschel, the chief characteristic of the sidereal system is *infinite variety*.

In order that the arguments on which these views are based may be clearly apprehended, it will be necessary to recall the main results of Sir Wm. Herschel's system of star-grouping.

Directing one of his 20-feet reflectors to different parts of the heavens, he counted the stars seen in the field of view. Assuming that the telescope really reached the limits of the sidereal system, it is clear that the number of stars seen in any direction affords a means of estimating the relative extension of the system in that direction, provided always that the stars are really distributed throughout the system with a certain approach to uniformity. Where many stars are seen, there the system has its greatest extension; where few, there the limits of the system must be nearest to us.

Sir Wm. Herschel was led by this process of star-grouping to the conclusion that the sidereal system has the figure of a cloven disc. The stars visible to the naked eye lie far within the limits of this disc. Stars outside the relatively narrow limits of the sphere including all the visible stars, are separately invisible; but where the system has its

greatest extension these orbs produce collectively the diffused light which forms the Milky Way.

Sir John Herschel, applying a similar series of researches to the southern heavens, was led to a very similar conclusion. His view of the sidereal system differs chiefly in this respect from his father's, that he considered the stars within certain limits of distance from the sun to be spread less richly through space than those whose united lustre produces the milky light of the galaxy.

Now it is clear that if the supposition on which these views are based is just, the three following results are to be looked for.

In the first place the stars visible to the naked eye would be distributed with a certain general uniformity over the celestial sphere; so that if on the contrary we find certain extensive regions over which such stars are strewn much more richly than over the rest of the heavens, we must abandon Sir Wm. Herschel's fundamental hypothesis and all the conclusions which have been based upon it.

In the second place, we ought to find no signs of the aggregation of lucid stars into streams or clustering groups. If we should find such associated groups we must abandon the hypothesis of uniform distribution and all the conclusions founded on it.

Thirdly, and most obviously of all, the lucid stars ought not to be associated in a marked manner with the figure of the Milky Way. To take an illustrative instance. When we look through a glass window at a distant landscape we do not find that the specks in the substance of the glass seem to follow the outline of valleys, hills, trees, or whatever features the landscape may present. In like manner, regarding the sphere of the lucid stars as in a sense the window through which we view the Milky Way, we ought not to find these stars, which are so near to us, associated with the figure of the Milky Way, whose light comes from distances so enormously exceeding those which separate us from the lucid stars. Here again, then, if there should appear signs of such association, we must abandon the theory that the sidereal system is constituted as Sir Wm. Herschel supposed.

It should further be remarked that the three arguments derived from these relations are independent of each other. They are not as three links of a chain, any one of which being broken the chain is broken. They are as three strands of a triple cord. If one strand holds, the cord holds. It may be shown that all three are to be trusted.

It is not to be expected, however, that the stars as actually seen should exhibit these relations, since far the larger number are but faintly visible; so that the eye would look in vain for the signs of law among them, even though law may be there. What is necessary is that maps should be constructed on a uniform and intelligible plan, and that in these maps the faint stars should be made bright, and the bright stars brighter.

The maps exhibited during this discourse have been devised for this purpose amongst others. There are twelve of them, but they overlap, so that in effect each covers a tenth part of the heavens. There is

first a north-polar map, then five maps symmetrically placed around it; again, there is a south-polar map, and five maps symmetrically placed round that map; and these five so fit in with the first five as to complete the enclosure of the whole sphere. In effect every map of the twelve has five maps symmetrically placed around it and overlapping it.*

Since the whole heavens contain but 5932 stars visible to the naked eye, each of the maps should contain on the average about 593 stars. But instead of this being the case, some of the maps contain many more than their just proportion of stars, while in others the number as greatly falls short of the average. One recognizes, by combining these indications, the existence of a roughly circular region, rich in stars, in the northern heavens, and of another, larger and richer, in the southern hemisphere.

To show the influence of these rich regions, it is only necessary to exhibit the numerical relations presented by the maps.

The north-polar map, in which the largest part of the northern rich region falls, contains no less than 693 lucid stars, of which upwards of 400 fall within the half corresponding to the rich region. Of the adjacent maps, two contain upwards of 500 stars, while the remaining three contain about 400 each. Passing to the southern hemisphere, we find that the south-polar map, which falls wholly within a rich region, contains no less than 1132 stars! One of the adjacent maps contains 834 stars, and the four others exhibit numbers ranging from 527 to 595.

It is wholly impossible not to recognize so unequal a distribution as exhibiting the existence of special laws of stellar aggregation.

It is noteworthy, too, that the greater Magellanic cloud falls in the heart of the southern rich region. Were there not other signs that this wonderful object is really associated with the sidereal system it might be rash to recognize this relation as indicating the existence of a physical connection between the Nubecula Major and the southern region rich in stars. Astronomers have indeed so long regarded the Nubeculæ as belonging neither to the sidereal nor to the nebular systems, that they are not likely to recognize very readily the existence of any such connection. Yet how strangely perverse is the reasoning which has led astronomers so to regard these amazing objects. Presented fairly, that evidence amounts simply to this: The Magellanic clouds contain stars and they contain nebulæ; therefore they are neither nebular nor stellar. Can perversity of reasoning be pushed farther? Is not the obvious conclusion this, that since nebulæ and stars are *seen* to be intermixed in the Nubeculæ, the nebular and stellar systems form in reality but one complex system.

As to the existence of star-streams and clustering aggregations, we have also evidence of a decisive character.† There is a well-

* It will be understood that the description here, and all which immediately follows, replaces portions of the discourse which would only be intelligible when illustrated by means of the diagrams and illuminated maps actually employed.

† Here, again, without the illustrative maps the argument is necessarily rendered imperfect.

marked stream of stars running from near Capella towards Monoceros. Beyond this lies a long dark rift altogether bare of lucid orbs, beyond which again lies an extensive range of stars, covering Gemini, Cancer, and the southern parts of Leo. This vast system of stars resembles a gigantic sidereal billow flowing towards the Milky Way as towards some mighty shore-line. Nor is this description altogether fanciful; since one of the most marked instances of star-drift presently to be adduced refers to this very region. These associated stars *are* urging their way towards the galaxy, and that at a rate which, though seemingly slow when viewed from beyond so enormous a gap as separates us from this system, must in reality be estimated by millions of miles in every year.

Other streams and clustering aggregations there are which need not here be specially described. But it is worth noticing that all the well-marked streams recognized by the ancients seem closely associated with the southern rich region already referred to. This is true of the stars forming the River Eridanus, the serpent Hydra, and the streams from the water-can of Aquarius. It is also noteworthy that in each instance a portion of the stream lies outside the rich region, the rest within it; while all the streams which lie on the same side of the galaxy tend towards the two Magellanic clouds.

Most intimate signs of association between lucid stars and the galaxy can be recognized,—(i.) in the part extending from Cygnus to Aquila; (ii.) in the part from Perseus to Monoceros; (iii.) over the ship Argo; and (iv.) near Crux and the feet of Centaurus.

Before proceeding to the subject of Star-drift, three broad facts may be stated. They are, I believe, now recognized for the first time, and seem decisive of the existence of special laws of distribution among the stars:—

First, the rich southern region, though covering but a sixth part of the heavens, contains one-third of all the lucid stars, leaving only two-thirds for the remaining five-sixths of the heavens.

Secondly, if the two rich regions and the Milky Way be considered as one part of the heavens, the rest as another, then the former part is three times as richly strewn with lucid stars as the second.

Thirdly, the southern hemisphere contains one thousand more lucid stars than the northern, a fact which cannot but be regarded as most striking when it is remembered that the total number of lucid stars in both hemispheres falls short of 6000.

Two or three years ago, the idea suggested itself to me that if the proper motions of the stars were examined, they would be found to convey clear information respecting the existence of variety of structure, and special laws of distribution within the sidereal system.

In the first place the mere amount of a star's apparent motion must be regarded as affording a means of estimating the star's distance. The nearer a moving object is, the faster it will seem to move,

and *vice versâ*. Of course in individual instances little reliance can be placed on this indication; but by taking the average proper motions of a set of stars, no untrustworthy measure may be obtained of their average distance, as compared with the average distance of another set.

For example, we have in this process the means of settling the question whether the apparent brightness of a star is indeed a test of relative nearness. According to accepted theories the sixth-magnitude stars are ten or twelve times as far off as those of the first magnitude. Hence their motions should, on the average, be correspondingly small. Now, to make assurance doubly sure, I divided the stars into two sets, the first including the stars of the 1st, 2nd, and 3rd, the second including those of the 4th, 5th, and 6th magnitude. According to accepted views, the average proper motion for the first set should be about five times as great as that for the second. I was prepared to find it about three times as great; that is, not so much greater as the accepted theories require, but still considerably greater. To my surprise I found that the average proper motion of the brighter orders of stars is barely equal to that of the three lower orders.

This proves beyond all possibility of question that by far the greater number of the fainter orders of stars (I refer here throughout to lucid stars) owe their faintness not to vastness of distance, but to real relative minuteness.

To pass over a number of other modes of research, the actual mapping of the stellar motions, and the discovery of the peculiarity to which I have given the name of star-drift, remains to be considered.

In catalogues it is not easy to recognize any instances of community of motion which may exist among the stars, owing to the method in which the stars are arranged. What is wanted in this case (as in many others which yet remain to be dealt with) is the adoption of a plan by which such relations may be rendered obvious to the eye. The plan I adopted was to attach to each star in my maps a small arrow, indicating the amount and direction of that star's apparent motion in 36,000 years (the time-interval being purposely lengthened, as otherwise most of the arrows would have been too small to be recognized). When this was done, several well-marked instances of community of motion could immediately be recognized.

It is necessary to premise, however, that before the experiment was tried, there were reasons for feeling very doubtful whether it would succeed. A system of stars might really be drifting athwart the heavens, and yet the drift might be rendered unrecognizable through the intermixture of more distant or nearer systems having motions of another sort, and seen accidentally in the same general direction.

This was found to be the case, indeed, in several instances. Thus the stars in the constellation Ursa Major, and neighbouring stars in Draco, exhibit two well-marked directions of drift. The stars β, γ, δ, ε, and ζ of the Great Bear, besides two companions of the last-named star, are travelling in one direction, with equal velocity, and clearly form one system. The remaining stars in the neighbourhood

are travelling in a direction almost exactly the reverse. But even this relation, thus recognized in a region of diverse motions, is full of interest. Baron Mädler, the celebrated German astronomer, recognizing the community of motion between ζ Ursæ and its companions, calculated the cyclic revolution of the system to be certainly not less than 7000 years. But when the complete system of stars showing this motion is considered, we get a cyclic period so enormous, that not only the life of man, but the life of the human race, the existence of our earth, nay, even the existence of the solar system, must be regarded as a mere day in comparison with that amazing cycle.

Then there are other instances of star-drift where, though two directions of motion are not intermixed, the drift character of the motion is not at once recognized, because of the various distances at which the associated stars lie from the eye.

A case of this kind is to be met with in the stars forming the constellation Taurus. It was here that Mädler recognized a community of motion among the stars, but he did not interpret this as I do. He had formed the idea that the whole of the sidereal system must be in motion around some central point; and for reasons which need not here be touched on, he was led to believe that in whatever direction the centre of motion may lie, the stars seen in that general direction would exhibit a community of motion. Then, that he might not have to examine the proper motions all over the heavens, he inquired in what direction (in all probability) the centre of motion may be supposed to lie. Coming to the conclusion that it must be towards Taurus, he examined the proper motions in that constellation, and found a community of motion which led him to regard Alcyone, the chief star of the Pleiades, as the centre around which the sidereal system is moving. Had he examined farther he would have found more marked instances of community of motion in other parts of the heavens, a circumstance which would have at once compelled him to abandon his hypothesis of a central sun in the Pleiades, or at least to lay no stress on the evidence derivable from the community of motion in Taurus.

Perhaps the most remarkable instance of star-drift is that observed in the constellations Gemini and Cancer. Here the stars seem to set bodily towards the neighbouring part of the Milky Way. The general drift in that direction is too marked, and affects too many stars, to be regarded as by any possibility referable to accidental coincidence.

It is worthy of note that if the community of star-drift should be recognized (or I prefer to say, *when* it is recognized), astronomers will have the means of determining the relative distances of the stars of a drifting system. For differences in the apparent direction and amount of motion can be due but to differences of distance and position, and the determination of these differences becomes merely a question of perspective.*

* Here no account is taken of the motions of the stars within the system; such motions must be minute compared with the common motion of the system.

Before long it is likely that the theory of star-drift will be subjected to a crucial test, since spectroscopic analysis affords the means of determining the stellar motions of recess or approach. The task is a very difficult one, but astronomers have full confidence that in the able hands of Mr. Huggins it will be successfully accomplished. I await the result with full confidence that it will confirm my views.

Turning to the subject of Star-mist, under which head I include all orders of nebulæ, I propose to deal but with a small proportion of the evidence I have collected to prove that none of the nebulæ are external galaxies. That evidence has indeed become exceedingly voluminous.

I shall dwell, therefore, on three points only.

First, as to the distribution of the nebulæ:—They are not spread with any approach to uniformity over the heavens, but are gathered into streams and clusters. The one great law which characterizes their distribution is an avoidance of the Milky Way and its neighbourhood. This peculiarity has strangely enough been regarded by astronomers as showing that there is no association between the nebulæ and the sidereal system. They have forgotten that marked contrast is as clear a sign of association as marked resemblance, and has always been so regarded by logicians.

Secondly, there are in the southern heavens two well-marked streams of nebulæ. Each of these streams is associated with an equally well-marked stream of stars. Each intermixed stream directs its course towards a Magellanic Cloud, one towards the Nubecula Minor, the other towards the Nubecula Major. To these great clusters they flow, like rivers towards some mighty lake. And within these clusters, which are doubtless roughly spherical in form, there are found intermixed in wonderful profusion, stars, star clusters, and all the orders of nebulæ. Can these coincidences be regarded as accidental? And if not accidental, is not the lesson they clearly teach us this, that nebulæ form but portions of the sidereal system, associating themselves with stars on terms of equality (if one may so speak), even if single stars be not more important objects in the scale of creation, than these nebulous masses, which have been so long regarded as equalling, if not outvying, the sidereal system itself in extent?

The third point to which I wish to invite attention is the way in which in many nebulæ stars of considerable relative brightness, and belonging obviously to the sidereal system, are so associated with nebulous masses as to leave no doubt whatever that these masses really cling around them. The association is in many instances far too marked to be regarded as the effect of accident.

Amongst other instances* may be cited the nebula round the stars c^1 and c^3 in Orion. In this object two remarkable nebulous nodules centrally surround two double stars. Admitting the association here to be real (and no other explanation can reasonably be admitted), we are led to

* Eight pictures of nebulæ were exhibited in illustration of this peculiarity.

interesting conclusions respecting the whole of that wonderful nebulous region which surrounds the sword of Orion. We are led to believe that the other nebulæ in that region are really associated with the fixed stars there; that it is not a mere coincidence, for instance, that the middle star in the belt of Orion is involved in nebula, or that the lowest star of the sword is similarly circumstanced. It is a legitimate inference from the evidence that all the nebulæ in this region belong to one great nebulous group, which extends its branches to these stars. As a mighty hand this nebulous region seems to gather the stars here into close association, showing us in a way there is no misinterpreting, that these stars form one system.

The nebula around the strange variable star, Eta Argûs, is another remarkable instance of this sort. More than two years ago I ventured to make two predictions about this object. The first was a tolerably safe one. I expressed my belief that the nebula would be found to be gaseous. After Mr. Huggins' discovery that the great Orion nebula is gaseous, it was not difficult to see that the Argo nebula must also be so. At any rate, this has been established by Captain Herschel's spectroscopic researches. The other prediction was more venturesome. Sir John Herschel, whose opinions on such points one would always prefer to share, had expressed his belief that the nebula lies far out in space beyond the stars seen in the same field of view. I ventured to express the opinion that those stars are involved in the nebula. Lately there came news from Australia that Mr. Le Sueur, with the great reflector erected at Melbourne, has found that the nebula has changed largely in shape since Sir John Herschel observed it. Mr. Le Sueur accordingly expressed his belief that the nebula lies *nearer* to us than the fixed stars seen in the same field of view. More lately, however, he has found that the star Eta Argûs is shining with the light of burning hydrogen, and he expresses his belief that the star has consumed the nebulous matter near it. Without agreeing with this view, I recognize in it a proof that Mr. Le Sueur now considers the nebula to be really associated with the stars around it. My belief is that as the star recovers its brilliancy observation will show that the nebula in its immediate neighbourhood becomes brighter (*not* fainter through being consumed as fuel). In fact, I am disposed to regard the variations of the nebula as systematic, and due to orbital motions among its various portions around neighbouring stars.

As indicative of other laws of association bearing on the relations I have been dealing with, I may mention the circumstance that red stars and variable stars affect the neighbourhood of the Milky Way or of well-marked star-streams. The constellation Orion is singularly rich in objects of this class. It is here that the strange 'variable' Betelgeux lies. At present this star shows no sign of variation, but a few years ago it exhibited remarkable changes. One is invited to believe that the star may have been carried by its proper motion into regions where there is a more uniform distribution of the material whence this

orb recruits its fires. It may be that in the consideration of such causes of variation affecting our sun in long past ages a more satisfactory explanation than any yet obtained may be found of the problem geologists find so perplexing—the former existence of a tropical climate in places within the temperate zone, or even near the Arctic regions.*

It remains that I should exhibit the general results to which I have been led. It has seemed to many that my views tend largely to diminish our estimate of the extent of the sidereal system. The exact reverse is the case. According to accepted views there lie within the range of our most powerful telescopes millions of millions of suns. According to mine the primary suns within the range of our telescopes must be counted by tens of thousands, or by hundreds of thousands at the outside. What does this diminution of numbers imply but that the space separating sun from sun is enormously greater than accepted theories would permit? And this increase implies an enormous increase in the estimate we are to form of the vital energies of individual suns. For the vitality of a sun, if one may be permitted the expression, is measured not merely by the amount of matter over which it exercises control, but by the extent of space within which that matter is distributed. Take an orb a thousand times vaster than our sun, and spread over its surface an amount of matter exceeding a thousandfold the combined mass of all the planets of the solar system :—So far as living force is concerned, the result is —*nil*. But distribute that matter throughout a vast space all round the orb :—That orb becomes at once fit to be the centre of a host of dependent worlds. Again, according to accepted theories, when the astronomer has succeeded in resolving the milky light of a portion of the galaxy into stars, he has in that direction, at any rate, reached the limits of the sidereal system. According to my views, what he has really done has been but to analyze a definite aggregation of stars, a mere corner of that great system. Yet once more, according to accepted views, thousands and thousands of galaxies, external to the sidereal system, can be seen with powerful telescopes. If I am right, the external star-systems lie far beyond the reach of the most powerful telescope man has yet been able to construct, insomuch that perchance the nearest of the outlying galaxies may lie a million times beyond the range even of the mighty mirror of the great Rosse telescope.

But this is little. Wonderful as is the extent of the sidereal system as thus viewed, even more wonderful is its infinite variety. We know how largely modern discoveries have increased our estimate of the complexity of the planetary system. Where the ancients recognized but a few planets, we now see, besides the planets, the families of satellites; we see the rings of Saturn, in which minute satellites must

* Sir John Herschel long since pointed to the variation of our sun as a possible cause of such changes of terrestrial climate.

be as the sands on the sea-shore for multitude; the wonderful zone of asteroids; myriads on myriads of comets; millions on millions of meteor-systems, gathering more and more richly around the sun, until in his neighbourhood they form the crown of glory which bursts into view when he is totally eclipsed. But wonderful as is the variety seen within the planetary system, the variety within the sidereal system is infinitely more amazing. Besides the single suns, there are groups and systems and streams of primary suns; there are whole galaxies of minor orbs; there are clustering stellar aggregations, showing every variety of richness, of figure, and of distribution; there are all the various forms of nebulæ, resolvable and irresolvable, circular, elliptical, and spiral; and lastly, there are irregular masses of luminous gas, clinging in fantastic convolutions around stars and star-systems. Nor is it unsafe to assert that other forms and varieties of structure will yet be discovered, or that hundreds more exist which we may never hope to recognize.

But lastly, even more wonderful than the infinite variety of the sidereal system, is its amazing vitality. Instead of millions of inert masses, we see the whole heavens instinct with energy,—astir with busy life. The great masses of luminous vapour, though occupying countless millions of cubic miles of space, are moved by unknown forces like clouds before the summer breeze; star-mist is condensing into clusters; star-clusters are forming into suns; streams and clusters of minor orbs are swayed by unknown attractive energies; and primary suns singly or in systems are pursuing their stately path through space, rejoicing as giants to run their course, extending on all sides the mighty arm of their attraction, gathering from ever new regions of space supplies of motive energy, to be transformed into the various forms of force,—light, and heat, and electricity,—and distributed in lavish abundance to the worlds which circle round them.

Truly may I say, in conclusion, that whether we regard its vast extent, or its infinite variety, or the amazing vitality which pervades its every portion, the sidereal system is of all the subjects man can study, the most imposing and the most stupendous. It is as a book full of mighty problems,—of problems which are as yet almost untouched by man, of problems which it might seem hopeless for him to attempt to solve. But those problems are given to him for solution, and he *will* solve them, whenever he dares attempt to decipher aright the records of that wondrous volume.

[R. A. P.]

GENERAL MONTHLY MEETING,

Monday, May 9, 1870.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

The following Vice-Presidents were nominated for the ensuing year:—

The Marquis of Salisbury.
Lieut.-Gen. Sir Edward Sabine, K.C.B.
Sir Roderick I. Murchison.
Warren De la Rue, Esq.
William Spottiswoode, Esq. Treasurer R.I.

Thomas William Boord, Esq. F.S.A.
Miss Eliza Bowman.
Miss Margaret Graham.
Rev. Brenchley Kingsford, M.A.
Henry Francis Makins, Esq.
Reginald Heber Prance, Esq.
The Earl of Rosse, F.R.S.
The Hon. Captain Reginald Talbot, M.P.
The Hon. Percy S. Wyndham, M.P.

were *elected* Members of the Royal Institution.

John Tyndall, Esq. LL.D. F.R.S. was re-elected Professor of Natural Philosophy.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

from

Lords of the Committee of Council on Education—Universal Catalogue of Works on Art. Vol. I. 4to. 1870.

Actuaries, Institute of—Journal, No. 99. 8vo. 1870.

Asiatic Society of Bengal—Journal. No. 158. 8vo.
Proceedings, 1869, No. 11. 1870, No. 2. 8vo.

Breen, Hugh, Esq. (the Author)—Corrections of Bouvard's Elements of Jupiter and Saturn. (Greenwich Observations. 1868.)

British Association for the Advancement of Science—Report of the Thirty-ninth Meeting at Exeter in 1869. 8vo. 1870.

Chambers, G. F. Esq. F.R.A.S. M.R.I.—Rev. C. S. Grueber, Plea for the Continuance of Primitive and Catholic Usages and Ornaments. (K 97) 8vo. 1870.

J. Wordsworth, Lectures Introductory to a History of the Latin Language and Literature. (K 97) 8vo. 1870.

Rev. S. C. Malan, Divine Liturgy of the Armenian Church of S. Gregory the Illuminator, translated from the Armenian. (K 97) 8vo. 1870

Chemical Society—Journal for April, 1870. 8vo.
Comitato Geologico d'Italia—Bolletino 3. 8vo. 1870.
Cracroft, Bernard, Esq. M.R.I. (*the Author*)—Bank Dividend Chart. 1869–70.
Dempsey, J. Maurice, Esq. (*the Editor*)—Our Ocean Highways: a Condensed Universal Route Book. 8vo. 1870.
Editors—Academy for April, 1870. 4to.
Artizan for April, 1870. 4to.
Athenæum for April, 1870. 4to.
Chemical News for April, 1870. 4to.
Engineer for April, 1870. fol.
Horological Journal for April, 1870. 8vo.
Journal of Gas-Lighting for April, 1870. 4to.
Mechanics' Magazine for April, 1870. 8vo.
Nature for April, 1870. 4to.
Pharmaceutical Journal for April, 1870. 8vo.
Photographic News for April, 1870. 4to.
Practical Mechanics' Journal for April, 1870. 4to.
Revue des Cours Scientifiques et Littéraires, April, 1870.
Scientific Opinion for April, 1870. 4to.
Franklin Institute—Journal. Nos. 529, 530. 8vo. 1870.
Geological Institute, Imperial, Vienna—Jahrbuch. Band XIX. No. 4. 8vo. 1869.
Verhandlungen, 1869. Nos. 14–18. 8vo.
Geological Society—Quarterly Journal, No. 102. 8vo. 1870.
Gladstone, J. H. Esq. Ph.D. F.R.S. M.R.I.—J. Miller, M.A. Christianum Organum. 16mo. 1870.
Institut Impérial de France, Académie des Sciences—Mémoires. Tomes 29, 32, 34, 35. 4to. 1864–67.
Mémoires par Divers Savants. Tomes 18, 19. 4to. 1865–68.
Jones, H. Bence, M.D. F.R.S. Hon. Sec. R.I. (*the Author*)—Life and Letters of Faraday. 2nd edition. 2 vols. 8vo. 1870.
Portrait of Professor Faraday (engraved from a Drawing by George Richmond, R.A., in 1852). 1870.
London Corporation Library Committee—Analytical Indexes to Vols. II. and VIII. of the 'Remembrancia,' 1593–1609; and 1613–1640. 8vo. 1870.
Mechanical Engineers' Institution—Proceedings. Jan. 1870. 8vo.
Meteorological Society—Proceedings, Nos. 47, 48. 8vo. 1870.
Musée Teyler, Haarlem—Archives. Vol. II. Fasc. 4. 4to. 1870.
National Education Union—Report of Congress at Manchester, Nos. 3, 4. 1869. 8vo. 1869.
Photographic Society—Journal, No. 216. 8vo. 1870.
Porter, J. H. Esq. (*the Author*)—The Sugar, Beet and Beet-root Sugar. (K 97) 8vo. 1870.
Preussische Akademie der Wissenschaften—Monatsberichte, Feb. 1870. 8vo.
Prussian Academy, Royal—Monatsberichte, Jan. 1870. 8vo.
Royal Society of London—Proceedings, No. 118. 8vo. 1870.
Philosophical Transactions for 1869, Vol. CLIX. Part 2. 4to. 1870.
Statistical Society—Journal, Vol. XXXIII. Part 1. 8vo. 1870.
Symons, G. J. Esq. (*the Author*)—Symons' Monthly Meteorological Magazine, April, 1870. 8vo.
Report of Rainfall Committee, 1868–69. 8vo. 1870.
Williams, C. J. B. M.D. F.R.S. (*the Author*)—The Case of the late Earl St. Maur. (K 97) 2nd edition. 8vo. 1870.

WEEKLY EVENING MEETING,

Friday, May 13, 1870.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

The Rev. Henry Moseley, M.A. F.R.S.
CANON OF BRISTOL AND INSTIT. IMP. SC. PARIS CORRESP.

On the Descent of Glaciers.

Glaciers do not take their origin in the highest Alpine regions. It is not there that the snow chiefly falls, but on a belt girding them below. This wide belt is divided horizontally into an upper and lower part by the snow-line, at a height of from 3000 to 3300 yards. Above that line snow always lies, and rain very rarely falls; beneath the snow-line the snow disappears every summer, and rains are abundant. It is from this belt about the snow-line that the glaciers are seen emerging. They lie like huge slugs along the descending valleys, swelling themselves out to fill their channels where they are wide, and thinning themselves to pass through the gorges and narrow places in them. They seldom come down to a lower level than 1100 yards. Between this level where they end and the snow-line, 3100 yards high, where they begin, they traverse sometimes a very long space—lying for the most part at a low pitch. The resemblance to a huge mollusk is kept up in *this*, that they move with a strange slow motion, not altogether unlike that of such an animal. The parallel will be complete if we conceive the mollusk to have its tail continually renewed as it withdraws it from under the snow-line, and its head continually melted away as it thrusts it forward below the level of from 1000 to 1400 yards. If we further imagine the steep sides of the valley through which the glacier descends to have similar but smaller glaciers crawling down them to the principal glacier, we shall understand what is meant by tributary or secondary glaciers, which are *thus* placed in regard to the principal ones; having a far greater pitch or slope than they, and flowing into them like tributary streams to a river. The slope of a principal glacier is often as little as 3°, and yet it may move with a velocity of 24 inches a day. The slope of a tributary glacier is sometimes 50°, and it may not advance more than 4 or 5 inches a day at the most.* Masses of rock of different sizes, from huge boulders to stones, are constantly broken by the frost from the sides of the valley of the glacier, and are carried slowly down on its

* The motion of the Glunberg, a tributary of the Aar glacier, inclined at 30° to 50°, was found by Desor to be 22 mètres a-year, while that of the Aar glacier, inclined at 4°, was 77 mètres.

back to the level where its head melts away, and there are deposited. These are called moraines. They lie along the course of the glacier in ridges protecting the ice beneath them from the sun's rays. That ice does not therefore melt as the rest of the ice does, and so it forms a ridge of ice. A moraine is therefore a ridge of stones standing on a ridge of ice.

The descent of a glacier is not a descent of the whole together, or bodily like that of a block of stone. There is an internal descent of every particle in the glacier over and alongside of every other particle. If a plane section be imagined to be made across it, the particles of ice passing through that cross-section at any given time must be conceived to be all moving at different rates so as to be sliding over and beside one another; the particles at the surface moving faster than those below, and the particles near the centre moving faster than those at a distance from it, exactly as the particles of a stream of water move.

The existence of this differential motion is strikingly seen in what is called the veined structure of glaciers. This veined structure appears first to have been described by M. Guyot, in the year 1838. The following is his account of it, as he saw it on the glacier of Gries, which I translate from the work of M. Huber:—

"I saw under my feet the entire surface of the glacier covered with furrows an inch or two wide cut in snowy ice, and separated from one another by ridges of harder and more transparent ice. It was evident that the mass of the glacier was here formed of these two different kinds of ice: the former (that of the furrows) was white and melted more rapidly; the other (that of the ridges) was more perfect, crystallized, transparent, and hard. The unequal resistance to melting of these two kinds of ice was the obvious cause of these depressions and elevations. After having followed them for several hundreds of mètres I reached a crevasse 20 or 30 feet wide, which, cutting the furrows at right angles, exhibited down to the depth of 30 or 40 feet an admirable transverse section of this structure. As far as my eye could reach I saw the mass of the glacier composed of layers of the [opaque] white ice separated from one another by layers of the transparent ice, the whole forming a mass as regularly stratified as certain calcareous rocks." This is the veined structure of glaciers.

[Canon Moseley's remarks on the differential motion of glaciers, with special reference to the observations and experiments of Forbes and Tyndall, illustrated by diagrams, will be found in the 'Philosophical Magazine' for April, 1870.]

The effect of the differential motion in *separating* and *distributing* the parts of a glacier has been shown in various ways. The remains of the guides lost in 1820 in Dr. Hamel's attempt to ascend Mont Blanc were found imbedded in the ice of the Glacier des Bossons in 1863. "The men and their things were torn to pieces and widely separated, many feet. All around them the ice was covered in every direction for 20 or 30 feet with the hair of one knapsack, spread over

an area three or four hundred times greater than that of the knapsack. This," says Mr. Cowell, from whose paper read before the Alpine Club in April, 1864, I have made the quotation, "is not an isolated example of the scattering that takes place in or on a glacier; for I have myself seen on the Glacier of Theodule the remains of the syndic of Val Tournache scattered over a space of several acres."

Whatever the force may be which causes the descent of glaciers, it must be chiefly expended in this constant displacement of the particles of the ice over one another and alongside one another, of which the veined structure affords the evidence, and to which is opposed everywhere that force of resistance which is called *shearing* force. By the property of ice called regelation, when any surface of ice so sheared is brought into contact with another similar surface it unites with it, so as to form of the two one continuous mass. This may be shown by experiment. If a cylinder of ice be placed in an apparatus suitable for the purpose and slowly sheared partly across, the new surfaces continually being brought in contact with one another in the act of shearing will not present the slightest appearance of separation, the ice being *there* as continuous as elsewhere, its molecules entering into precisely the same relations to one another in their new positions as they did in the positions out of which they have been sheared. Thus a slow displacement of shearing, by which different similar surfaces of ice in a glacier were continually being brought into presence and contact with one another, would exhibit all the phenomena of the motion of glacier ice.

The late Principal Forbes calculated that in the Aar glacier the shearing displacement amounts to $\frac{3}{4}$ths of the whole displacement of the glacier. The sliding displacement of the whole glacier bodily on its bed is, in comparison with it, unimportant as it regards the expenditure of force requisite to bring it about. The differential motion is the *great* and *characteristic* phenomenon of the descent of glaciers; but it is that, in assigning an adequate cause for which, existing glacier theories seem to me most conspicuously to have failed.

The Swiss philosopher De Saussure was the first to study the descent of glaciers with care, and wrote on it about sixty years ago. He held that glaciers slip down the slopes on which they rest by their weight, just as other bodies slip down inclined planes. This explanation is simple, and was generally accepted as long as it was thought that glaciers slipped down bodily like blocks of stone would, with an equal motion of all their particles; but when the internal motion of their particles upon one another, like that of running water, came to be discovered, and when it was found that the high-pitched tributary glaciers moved slower than the low-pitched principal ones, this theory was brought into doubt, for it was in direct contradiction to these facts.

M. Rendu, the Bishop of Annecy, considered the descent of a glacier to be so like that of a fluid that it was impossible to explain it otherwise than by supposing ice actually *to be a fluid* and not the *solid* thing it seems to be. He was the founder of the celebrated viscous

theory of the descent of glaciers, advocated with such remarkable energy, industry, and ability by the late Principal James Forbes, whose various works on glaciers have exhausted the whole field of observation and supply most of the facts on which the true solution of the problem, whenever it is arrived at, must be founded. When, however, at another stage of the inquiry, it came to be discovered by Faraday and Tyndall that ice, when broken up, was capable of being united again by sufficient pressure, so as to become as perfectly solid and homogeneous as it was before, it became evident that supposing a sufficient pressure to be exerted on the glacier, in the direction of its descent, to crush its substance through the contractions and gorges of its channel, and over the irregularities in its bed, it would re-form itself and solidify, and become a compact mass again as it was before, when it had passed these obstructions. This is the regelation theory.

At this stage the question had assumed this new form—"If ice be a viscous fluid, according to the viscous theory, is it fluid *enough* to descend by its own weight; or, if it be a solid, according to the *regelation* theory, is it *little* enough solid so to descend?"

If, instead of ice, a glacier were of water, it *would* obviously descend by its weight. The same would be true if it were of oil, or soft mud, or quicksilver, or probably of pitch; but if it were of iron, or of copper, or of lead, it would not descend by its weight only, unless, indeed, these metals were in a state of fusion. A quicksilver glacier would descend by its weight only because it shears easily; a cast-iron one would not, because it shears with difficulty. There must, therefore, exist a relation between the shearing force and the weight of a given volume of a glacier, so that it may just descend by its weight only. Now, it is possible to investigate mathematically what that relation is.

I have made that investigation.* I have founded it on this well-known law of mechanical philosophy, that "The aggregate WORK of the forces which produce the displacement of a body or a system of bodies (however related) must at least equal the aggregate work of the resistances which oppose that displacement."

The resistances opposed to the displacement of a glacier are—1stly, Those which oppose themselves to the shearing of one surface of ice over another, which is continually taking place throughout the whole mass by reason of the differential motion; 2ndly, The friction of the superimposed laminæ of ice upon one another, which is greater in the lower than the upper; 3rdly, Abrasion of the ice on the bottom and sides of the channel of the glacier. If it descends by its weight only, then the work of its weight in its descent through any distance must at least equal the sum of the works of all these resistances. It is of course impossible to represent this relation mathematically in respect to an actual glacier having a variable direction and an irregular channel and slope; but in respect to an imaginary one having a constant direction and a uniform channel and slope, it is possible. I

* 'Phil. Mag.,' May, 1869.

have made that calculation, and it results from it that the unit of shear in ice (that is, the force necessary to make one square inch of ice shear over another square inch) must not be more than $1\frac{1}{3}$ lb. that a glacier may descend by its weight only. If the unit of shear in ice be more than that, then the glacier cannot descend by its weight only on a slope like that of the Mer de Glace. But it is a great deal more than that. It requires from 60 lbs. to 120 lbs. to shear one square inch of ice over another square inch. The ice of the Mer de Glace cannot therefore descend by its weight only; it does not shear easily enough. It must be ice of about the consistency of soft putty to descend by its weight only, for that substance shears with a pressure of from $1\frac{1}{2}$ lb. to 3 lbs. per square inch.

Ice, therefore, if it be fluid, is not fluid *enough*, and if it be solid, it is *too* solid to descend by its weight only. There must be some other force to help it down besides its weight—certainly forty-five times greater, and possibly ninety times. The imaginary case, to which alone these calculations apply, differs indeed from the actual case of a glacier in this respect, that it is straight and uniform. But if its weight be insufficient to bring a glacier down a straight and uniform channel, much more will it be so to bring it down a crooked and variable one. This result is directly opposed to the viscous theory and to that known as the theory of regelation, both of which attribute the descent of glaciers to their weight as the only cause. It reveals the existence of some other force. What is it?

All Alpine travellers, from De Saussure to Forbes and Tyndall, have borne testimony to the intensity of the solar radiation on the surfaces of glaciers. "I scarcely ever," says Forbes, "remember to have found the sun more piercing than at the Jardin." This heat passes abruptly into a state of intense cold when any part of the glacier falls into shadow by an alteration of the position of the sun, or even by the passing over it of a cloud. Now the Mer de Glace moves faster by day than by night. Its mean daily motion is twice as great during the six summer as during the six winter months. The connection between its rate of motion and the external temperature is most remarkable. It has been carefully observed, and the results as recorded by Professor Forbes leave no doubt of the fact, that no change of external mean temperature is unaccompanied by a corresponding change of glacier motion. From this it follows that the two are either dependent on some common cause, or that the one set of changes stands in the relation of a cause to the other. That both sets of phenomena—the changes of the sun's heat, and the changes of glacier motion—should be due to some common independent cause, seems impossible. We are forced therefore on the conclusion that one is caused by the other. And as the changes in the glacier motion cannot cause the changes of solar heat, it must be the changes of solar heat which cause the changes of glacier motion. The *sun* is obviously *master* of the glacier. Nor is this to be considered a startling or improbable conclusion. Heat is but another form of mechanical power. This power is constantly

streaming into the glacier; for ice is dia-thermanous. It is readily penetrable by luminous heat. This has been shown by Tyndall, who, having sent a beam of heat through a block of Wenham Lake ice, saw its course starred by the dilatations of the ice. I have, moreover, myself obtained an ice lens by causing ice to be turned in a lathe to a spherical surface by means of a templet of iron. Through this lens the rays of the sun streamed in abundance, and were concentrated in its focus with such intensity as to burn the hand and instantly set fire to a match. There can be no doubt therefore that the rays of the sun, which in those Alpine regions are of such remarkable intensity, find their way into the depths of the glacier. They are a *power*, and there is no such thing as the loss of power. The mechanical "work" which is their equivalent, and into which they are converted when received into the substance of a solid body, accumulates and stores itself up in the ice under the form of what we call elastic force, or tendency to dilate, until it becomes sufficient to produce actual dilatation of the ice in the direction in which the resistance is weakest, and by its withdrawal to produce contraction. How much heat entering the surface of a glacier is necessary to this result has been made the subject of calculation. Supposing the depth of the ice to be the same as that at the Tacul, its motion at different depths that which Tyndall found it to be there, and its surface motion that which he measured lower on the Mer de Glace at Les Ponts, and supposing the resistance to shearing of ice to be 75 lbs. per square inch; then the mechanical work, which acting within the mass is necessary to put the glacier in motion, as it actually moves, is $61\frac{3}{4}$ units of work per square inch of the surface of the glacier per day.* Now this quantity of work would be supplied by ·0635 heat-unit entering the ice per square inch of surface per day and diffusing itself through it, each heat-unit being the heat necessary to raise 1 lb. water by 1° F. Far more than this heat probably passes the surface and enters into the ice of a glacier on days such as that when the motion was observed which serves as the basis of these calculations.

But *how* does this mechanical power, which thus streams continually into a glacier when the sun shines on it, compel its descent?

The fact of the descent of a sheet of lead when placed upon the inclined surface of a roof, however low the pitch, especially in summer, has long been known; I myself first observed it on the southern side of the roof of the Choir of the Bristol Cathedral, in 1855. I have verified it by the following experiment:—I fixed a deal board 9 feet long and 5 inches broad to the southern wall of my house, so as to form an inclined plane, and upon it I placed a sheet of lead, turning its edges down over the side edges of the board, and taking care that it should not bind upon them, but be free to move with no other obstruction than that which arose from its friction. The inclination of the board was 18° 32′, the thickness of the lead $\frac{1}{8}$th of an inch, its length 9 feet, and its weight 28 lbs. The lower end of the board was

* 'Proceedings of the Royal Society,' January, 1869, p. 207.

brought opposite to a window, and a vernier was constructed, which could be read from within, and by which the position of the lead upon the board could be observed to the 100th of an inch. I began to measure the descent of the lead on the 16th of February, 1858, and recorded it every morning between seven and eight o'clock, and every evening between six and seven o'clock, until the 28th of June. I have preserved all these observations. In the night, between sunset and sunrise, the lead scarcely descended at all. It was on days when the thermometer in the sun varied its height rapidly and much—as on bright days with cold winds, or when clouds were driven over the sun —that the descent was greatest. So remarkably, indeed, was this the case, that every cloud which shut off the sun for a time from the lead, and every gust of wind which blew upon it in the sunshine, seemed to bring it down a step. On such days it would descend from $\frac{1}{4}$ to $\frac{1}{2}$ an inch. On the contrary, when the sky was open and clear, and the heat advanced and receded uniformly, the descent was less, although the difference of the extreme temperatures of day and night might be greater. It was least of all on days of continuous rain. The sun was the obvious cause of the descent of the lead. It was master of the lead, as it is of the glacier. A dilatation and contraction of it was caused by the passage into it, and the withdrawal, of the sun's radiant heat, and this dilatation and contraction of the lead caused it to descend. Why it should do so may be easily explained.

Let A B, Fig. 1, be an elementary plate of the solid, and conceive it to be divided into an infinite number of equal elements by planes perpendicular to its length. Let X be a point so taken in it that, if it were divided in X, the thrust necessary to push the part X A up the plane would equal that necessary to push X B down it. Let an element at X be imagined to have its temperature so raised as just to equal this thrust; and let the temperatures of all the elements in X A, beginning at X, be equally raised in succession. Each will thus be dilated more than the one before it, because its dilatation will be opposed by a less resistance; and the displacement $A A_1$ of the extremity upwards will equal the sum of these several dilatations. In like manner,

Fig. 1.

if the same temperature be added to the elements of X B in succession, beginning from X, each will be dilated more than the one before it, and the displacement $B B_1$ of the extremity B downwards will equal the sum of these several dilatations. The point X will obviously be

nearer to A than to B, because the same thrust of dilatation of the element at X would not be able to push so great a length of the bar up the plane as it would down it.

In this state of the temperature of the plate, let a point X_1 be taken such that, if it were divided there, the strain necessary to pull the part $X_1 A_1$ down the plane would just equal that necessary to pull $X_1 B_1$ up it. Let the temperature of the element at X_1 be so diminished as by its contraction just to produce this strain, and let the temperatures of all the elements from X_1 to A_1 in succession be similarly reduced. Each will contract more than the one before it, because a less resistance will be offered to its contraction; and the displacement $A_1 A_2$ of A_1 down the plane will equal the sum of these separate contractions. In the same way the displacement $B_1 B_2$ of B_1 up the plane will equal the sum of the separate contractions of the elements of $X_1 B_1$. The point X_1 will be farther from A_1 than B_1, because the same strain of contraction of an element at X_1 would pull a greater length of the bar down the plane than up it.

It is by the dilatation of the greater length of the plate X B favoured by its weight that the extremity B is displaced down the plane when the temperature is raised; whilst it is by the contraction of the less length $X_1 B_1$ against its weight that it is displaced up the plane when the temperature is lowered. The extremity B is therefore more displaced down the plane by a given raising of the temperature than it is displaced up it by a corresponding lowering. On the whole, therefore, the extremity B is made to *descend* the plane to B_2 by a given alternation of temperature. It is by the dilatation of the less length X A that the extremity A is displaced up the plane, and by the contraction of the greater length $X_1 A_1$ that it is displaced down the plane. It is therefore less displaced up by dilatation, than it is down by contraction, and on the whole it descends to A_2 by a given alternation of temperature. Both the extremities A and B of the plate are therefore made to descend when it is subjected to a given elevation and then to a corresponding depression of its temperature; that is, the *whole* plate is made to *descend* from A B to $A_2 B_2$.

Now, the theory of the descent of glaciers, which I have ventured to propose, is that they descend, as the lead in this experiment does, by reason of the passage into them and the withdrawal of the sun's rays, and that the dilatation and contraction of the ice so produced is the proximate cause.

There is no substance the dilatation or contraction of which, by changes of temperature, is more thoroughly and accurately known than that of ice. Experiments were made upon it by three independent observers, at the observatory of Pultowa, in the winters of 1845 and 1846, between the temperatures of 0·9 R. and 28·82 R., from which it resulted that ice is by far the most dilatable of all known solid substances, being nearly twice as dilatable as lead. These experiments have been described by Baron W. Struve, in the 'Transactions of the Academy of St. Petersburg.'

The ice of a glacier behaves itself in its descent exactly as the lead did in my experiment.

It has been argued in opposition to this theory that the temperature of the ice of glaciers is, by the observations of Agassiz, but very little below 32°, and that if radiant heat found its way into it, it would not expand, but melt it. To this there is the obvious answer that radiant heat does find its way into ice as a matter of common observation, and that it does not melt it, except at its surface, and therefore that it *does* dilate it. Blocks of ice may be seen in the windows of ice shops with the sun shining full upon them, and melting nowhere but on their surfaces. And the experiment of the ice-lens shows that heat may stream through ice in abundance—of which a portion is necessarily stopped in the passage—without melting it, except on its surface. My theory supposes that the ice beneath the surface of a glacier is a *solid*. That is all.

Being a solid, and receiving into its substance heat—that work—in great quantities, that work (by the principle of the conservation of force) cannot but be stored up in it, under the form of "potential energy," and wherever it is present, produce a tendency to dilatation. When that tendency takes effect, it is in the direction of the least resistance to the dilatation of that part. It matters not whether the temperature of the ice be below 32° or above it, provided only that the condition of solidity be satisfied. Being a solid, it cannot but dilate and contract under the variations of temperature to which it is subjected; and dilating and contracting, it cannot but descend. To show that the ice of a glacier does not dilate when heat is received into it, it would be necessary to show it not to be subject in this respect to a law common to all other solid bodies.

Great alternations of temperature are not necessary to cause the motion of a glacier. A succession of *small* alternations produces the same effect as one great one. Their effect is cumulative. Alternations backwards and forwards, of 2° each, six times repeated, would carry the glacier (under certain assumed conditions) as far down as a single alternation of 12°.

The observations of Agassiz on the temperature of the ice of the Aar glacier in 1841, 1842, were made in borings from 15 to 200 feet deep. Thermometers placed in these borings never fell below 0·3 C., although the external temperature descended at night to 5° or 6° C. Commonly they showed exactly zero.* Nothing can, however, be concluded from these experiments, because the thermometers were not *frozen into the ice* of the glacier, or the mouths of the borings so effectually stopped as to prevent the access of air or the percolation of water from the disintegrated ice near the surface. The maintenance of a constant state of humidity in the air of the boring not being thus provided against, the included thermometer could not but remain at zero, however low might be the temperature of the surrounding ice.

* 'Bulletin de Genève,' tom. 44, p. 349.

For the water of the contained air freezing on the sides of the boring (like hoar frost) would raise the temperature around the bulb, by the latent heat set free in freezing, to zero, C. And the humidity of the air being continually renewed, this process would always go on.

"A thermometer is, in short, incapable of taking the temperature of ice, unless that ice be dry."* These are the words of that eminent man Principal Forbes in the last (I think) of his papers on the subject of glaciers.

On a Friday evening like *this*, eight years ago, another eminent man, whose loss to science we all deplore, addressed the audience then assembled here on the subject on which I have now been addressing you. He possessed every accomplishment for the investigation of it, and had studied it long and successfully; he was versed in physics, and was an able and original mathematician. I speak of Mr. William Hopkins, whose name will always be held in honour by reason of his many contributions to the science of glaciers, and especially because of the explanation he first gave of the formation of crevasses. In the introductory part of the discourse which Mr. Hopkins then delivered he "insisted on the necessity of a more exact definition of terms and more accurate modes of mathematical reasoning than those which had up to that time characterized the discussion of glacial phenomena."

That, on the necessity of which he insisted and for which he laboured, I also have laboured for. I have sought to bring the discussion of glacial phenomena out of the wide region of scientific opinion, and place it in that of exact science. That work is, however, still far from being completed. Life, too, is short, and the power to pursue studies such as these is sometimes shorter in duration than life itself. But other workers are behind.

[H. M.]

WEEKLY EVENING MEETING,

Friday, May 20, 1870.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. **President**, in the Chair.

PROFESSOR ALEXANDER WILLIAMSON, F.R.S.

PRESIDENT OF THE CHEMICAL SOCIETY,

On Atoms.

[Abstract deferred.]

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* Forbes, 'Phil. Mag.,' March, 1859.

WEEKLY EVENING MEETING,

Friday, May 27, 1870.

WARREN DE LA RUE, Esq. Ph.D. F.R.S. Vice-President, in the Chair.

J. W. DAWSON, LL.D. F.R.S. &c.
PRINCIPAL OF M'GILL UNIVERSITY, MONTREAL.

On the Primitive Vegetation of the Earth.

THE existing flora of the earth is not primitive or primeval, but the last of a long series of dynasties of plants, extending from the oldest geological periods to the present time. Geology reveals to us several Tertiary floras, which perished before the modern period. These were preceded by the floras of the Mesozoic and these by those of the Palæozoic. Our attention will be directed this evening to the oldest of the Palæozoic floras, that of the Devonian and Upper Silurian periods.

To understand this old flora, it will be necessary for us to glance at the general divisions of the vegetable kingdom at present, and their range in geological time. Modern plants admit of a twofold division, into Phænogamous or flowering plants, and Cryptogamous or flowerless plants. The former have true seeds and fruits, the latter multiply by minute spores. Each of these great groups admits for our present purpose of a threefold division. Under Phænogams we have (1) *Angiospermous Exogens*, having stems with bark, wood and pith, and beginning their existence with two cotyledons, or seed leaves. These plants largely predominate among our trees, shrubs, and herbaceous plants, especially in the temperate climates. (2) *Endogens*, destitute of the divisions of the stem above mentioned, and beginning life with one cotyledon. The Palms belong to this group, and also the Grasses. (3) *Gymnosperms*, having exogenous stems, but with wood of more simple structure than in the first group, and having the organs of fructification of very imperfect character. This group includes the Pines and Cycads. Under Cryptogams we have also three divisions:—(1) *Acrogens*, the Ferns, Club-mosses, and Horse-tails. (2) *Anophytes*, the Mosses and their allies. (3) *Thallophytes*, the Lichens, Fungi, and Sea-weeds.

If we trace these forms of vegetation backward in geological time, we find that some of them appear to be older than others. The true Exogens, dominant in the modern time, continue to be so in the

Tertiary period; and in this the Endogens also exist as at present. As we pass backward in the Mesozoic, these two groups lose their importance, and we reach what has been called the age of Gymnosperms—Pines and Cycads being predominant. At the same time we lose from the other end of the scale the Anophytes. The Thallophytes scarcely at all appear as fossils, except in the case of a few Algæ. Thus in the Mesozoic flora the lowest group of the Phænogams and the highest group of the Cryptogams meet together, and share the world between them, almost to the exclusion of other groups.

Passing bakcward into the Palæozoic, the Cycads, which were plentiful in the Mesozoic, appear to fail us. We still find Conifers or Pine trees and other Gymnosperms, and we have a strange and remarkable development of the more humble Lycopodiaceous and Equisetaceous plants, the Club-mosses and Horse-tails, into arboreal forms. This condition of things exists in the Carboniferous period, in which our great beds of coal were formed, and extends backward into the oldest period in which we have a land flora. The Devonian thus belongs to the age of Acrogens, though it is not destitute of other forms.

My attention was first directed to these ancient plants by the discovery made many years ago by Sir W. E. Logan, that in the Gaspé sandstones, belonging to this group, there are not only drifted fragments of plants, but others rooted in the beds of clay or shale which evidently formed the soils in which they grew. On careful study the plants most abundant in these root-beds proved to belong to a type of vegetation previously unknown, and indicated by the restoration in Fig. 1. The lower portion consists of a cord-like rhizoma or root-stock, having a scalariform axis and a thick cellular bark. These rhizomata fill the root-beds with a confused mat of long cords, giving off numerous slender roots in every direction. In one case they have accumulated to a sufficient extent to form a bed of coal of one or two inches in thickness, the oldest known in America. The stems of these plants, to which the name *Psilophyton princeps* was given, are slender and branching, and in certain states of preservation are sparsely covered with short, stiff, spine-like leaves. In other states they are nearly smooth, with mere points instead of leaves. The young stems terminate in crozier-like circinate curls, like those of young ferns, and the fructification consists of oval sacs or spore-cases, borne gracefully on slender bending stalks. The fruit of these plants reminds the botanist of that of the modern Pillworts, but the stems are of more complete organization, and equal in this respect to those of modern Club-mosses. Such combinations of character are not unusual in fossil plants, and illustrate what seems to be a general law, *viz.* that in the older geological periods low forms of life were allowed to assume characteristics subsequently limited to higher grades of being.

The plants of the genus Psilophyton, of which there are several species, probably grew on swampy flats liable to inundation. They constitute the most characteristic and abundant members of the Lower Devonian flora, and appear to have been equally abundant in

Europe as in America, though when occurring in fragments they have often been mistaken for Algæ and for roots.

FIG. 1.

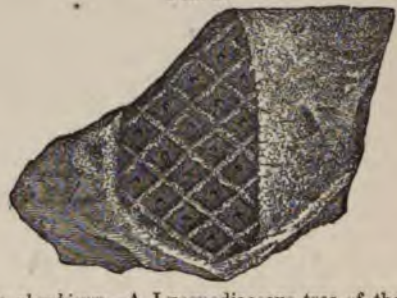
Psilophyton princeps, the oldest known plant of America, restored.

a, Fruit, natural size; *b*, stem, natural size; *c*, Scalariform tissue of the axis, highly magnified. In the restoration, one side is represented in vernation, and the other in fruit.

More distinctly allied to the modern Club-mosses were the *Lepidodendra*, *Leptophlea*, and similar plants of the Devonian, which may be defined to be gigantic arborescent Club-mosses or Lycopodiums. These also are widely diffused in the Devonian of all parts of the world; and some of the genera, especially Lepidodendron, are still more abundant

and attain to greater dimensions in the Carboniferous period. Fig. 2 represents a peculiar Devonian type, *Leptophleum rhombicum*.

Fig. 2.



Leptophleum rhombicum—A Lycopodiaceous tree of the Devonian. Portion of a branch, natural size.

Still another feature of this ancient vegetation was the occurrence of dense brakes of *Calamites*, plants which were exaggerations of the modern Equisetums, attaining to a diameter of several inches, and to a great height, and in some cases with strong and woody stems. The Calamites of the Devonian much resemble those of the Carboniferous, and two of the species are found also in the latter system. Probably allied to the Calamites were the beautiful Star-leaves of the genus *Asterophyllites*, which occur spread out on the Devonian shales of St. John, New Brunswick, as if prepared by a careful botanist.

The Ferns are among the most beautiful plants of the modern world in point of foliage, and they are very ancient in regard to geological time, making their appearance abundantly in the Middle

Fig. 3.

Cyclopteris Jacksoni—An American representative of *C. Hibernicus*, natural size.

Devonian. Of the Devonian species, few extend into the Carboniferous; and some forms, like that of the well-known *Cyclopteris* (*Archæopteris*) *Hibernicus* of the Upper Devonian of Ireland, are quite peculiar to that system. This type of ferns is represented in America by *Cyclopteris Jacksoni* and others, Fig. 3.

There seems to have been in the Devonian a prevalence of forms of ferns allied to the modern genera *Hymenophyllum* and *Trichomanes*. There were also tree-ferns, large trunks of which, of the genera *Psaronius* and *Caulopteris*, have been found by Professor Newberry in Ohio, and by Professor Hall in New York. Those of the former genus had the same habit found in the modern ferns, of sending out masses of aërial roots which, stretching into the soil like cords, supported the stem. In Scotland a small trunk, approaching to a tree-fern, has been found. It is the *Caulopteris Peachii* of Salter.

Of all the plants of the Palæozoic forests, the most singular are the *Sigillariæ*. The head-quarters of this genus, or family, are in the Carboniferous, but several species occur in the Devonian, though they are of comparatively small stature, as far as yet known. The Sigillariæ had tall slender stems, with vertical rows of narrow two-nerved leaves, and their structures present remarkable points of resemblance to those of modern Gymnosperms, though there is reason to suspect that some trees included in the group may have been Cryptogams of high organization. The Sigillariæ were the trees most important in the accumulation of coal in the Carboniferous; but in the Devonian we have no evidence that their remains accumulated to so great an extent. In the Lower Devonian especially, the rhizomata of *Psilophyton* appear to take the place which the Stigmaria roots of Sigillaria hold in the Carboniferous.

Rising still higher in the vegetable kingdom, we reach unquestionable Gymnosperms in the pine-trees of the genus *Dadoxylon*, whose drifted trunks, preserved in stone by the infiltration of silicious or calcareous matter, occur in the sandstones of New York, Ohio, and New Brunswick, and also in Scotland and Germany. The most noteworthy point with reference to these trees is that, while specifically distinct from those of the Carboniferous, they resemble them precisely in the structure of the woody fibres, and more especially in the beautiful bordered pores of the cell-walls, a structure fitted to promote the more free flow of the sap, and in which they agree with the fossil Conifers of the coal-formation, and also with the modern Araucarian pines of the southern hemisphere, but differ in detail from the modern pines of the northern hemisphere.

Pines of the Araucarian type are, as yet, limited in their downward range to the Middle Devonian. In the lower beds of the system we find trunks of drifted trees in the sandstones, at first sight resembling those of Dadoxylon; but their microscopic structure is quite different. They present a regular tissue of long cylindrical fibres, marked on their sides with irregular spiral lines, and very distinct from those of modern Conifers, though their markings suggest the

spiral lines on the cells of the Yews, whence I have taken the name *Prototaxites* for these remarkable trunks. They have medullary rays and regular lines of growth, and attained sometimes a diameter of three feet. Unfortunately we know nothing of their foliage or fruit, and can but suppose that they constitute a prototype of the coniferous trees, probably very different from any known in the modern world. Fig. 4 represents a fragment of a trunk of this oldest known tree, preserved by the infiltration of its tissues with silica.

Fig. 4.

Prototaxites Logani—Fragment of a trunk, much reduced.

But for one little fragment, we should have been ignorant of the existence in the Devonian of any higher type of tree than those above mentioned. In Professor Hall's collections, however, from Eighteen-Mile Creek, in Ohio, there is a small piece of wood mineralized in a similar way with other specimens found at that place, and showing, in addition to woody fibres and medullary rays, the large tubes or ducts characteristic of the modern angiospermous Exogens. No such wood has as yet occurred in the Carboniferous; and that it should be found in the Devonian is a startling fact, from which many geologists may be disposed to withhold their belief until it shall be confirmed by additional specimens. In the meantime I must place this remarkable tree, *Syringoxylon mirabile*, in the list of Devonian plants. Nothing further is known of it than the fragment of wood above mentioned. In accordance with the law already stated, it may admit of doubt whether its flowers and fruit would be of as high a type of organization as the wood, though it certainly must have ranked higher than the Gymnosperms.

The plants above mentioned are but examples of the old Devonian flora. It contains many other forms equally interesting to the geologist and botanist; but for these I must refer to my more extended memoirs.

In turning to the question whether any land flora comparable with that of the Devonian existed in still older periods, we may first consider the growth of our knowledge of the former, and its relation to

that of the preceding Carboniferous period. Twenty years ago scarcely anything was known even by those engaged in the study of vegetable fossils, of land plants older than the great coal-formation. In 1860 Goeppert, in his memoir on the plants of the Silurian, Devonian, and Lower Carboniferous rocks, mentions only one land plant, and this of very doubtful character, in the Lower Devonian; in the Middle Devonian he knew but one species; in the Upper Devonian he enumerated fifty-seven. Most of these were European, but he included also such American species as were known to him. My first paper on these plants was published in 1859, and had not reached Goeppert when his memoir was written. This, with other American facts, would have added ten or twelve species to his list, some of them Lower Devonian. In the ten years from 1860 to the present time, I have been able to raise the Devonian Flora of Eastern America to 121 species; and reckoning that of Europe at half that number, we now have at least 180 species of land plants from the Devonian, or perhaps about one-fifth of the number known in the Carboniferous, whose fossil flora is so copious, and has been so much longer known and so thoroughly explored. The Devonian flora includes about twenty-five of the more important generic or sub-generic forms recognized in the Carboniferous, and about the same number of forms not recognized in that system. Its species are on the other hand almost wholly distinct from those of the Carboniferous. The differences are, perhaps, chiefly such as might be connected with the less amount and more varied surface of the land in the earlier of the two periods.

The flora of the Upper Silurian is at present nearly in the same condition with that of the Middle and Lower Devonian ten years ago. I know in the Upper Silurian of Canada only two species of *Psilophyton*, both apparently identical with Devonian forms. In England, besides the spore-cases (*Pachytheca*) from the Ludlow, there exist in the collections of the Geological Survey fragments of wood and bark, which may indicate two or three additional species; one of them having a microscopic structure similar to, if not identical with, that of *Prototaxites*. In Germany three or four species are known in rocks of this age. All of these appear to be Acrogens allied to Lycopodiaceæ. That these few species constitute the whole of the Upper Silurian flora we can scarcely believe. They occur in marine formations, and were probably drifted far from the somewhat limited land-surfaces of the period. Could we approach more nearly to these shores, or find the deposits made by streams flowing from them, we might hope to find additional plants, and judging from those that we know, these would resemble the flora of the Lower Devonian.

With the exception of some remains which I believe to be of very doubtful character (the so-called *Eophyton* of Sweden and of Wales), the Lower Silurian has as yet afforded no remains of land plants; and in North America at least, where we have in this horizon the wide shallow sand-flats of the Potsdam sandstone, we might have expected to find some drift trees, had such existed. Still the search is not hope-

less; and the estuary of some stream flowing from the ancient Laurentian land, may still retain for us the plants of the Lower Silurian.

Lastly, for reasons stated in a paper lately published in the Proceedings of the Geological Society, I believe that the immense deposits of graphite in the Laurentian of Canada, represent carbon of vegetable origin, and probably in part produced by land plants, as yet wholly unknown to us in any other way. As the Palæozoic was the age of Acrogens, the Laurentian may have been that of Anophytes and Thallophytes. Its forests may have consisted of gigantic mosses and lichens, towering to the height of trees, and presenting us with a phase of vegetable existence bearing the same relation to that of the Palæozoic which the latter does to that of more modern periods. But there is another and more startling possibility: that the Laurentian may have been a period when vegetable life culminated on the earth, and existed in its most complete and grandest forms, in advance of the time when it was to be brought into subordination to the higher life of the animal. These are questions the solution of which belongs to the geology of the future.

To the modern naturalist, vegetable life, with regard to its uses, is the great accumulator of pabulum for the sustenance of the higher forms of vital energy manifested in the animal. In the Palæozoic this consideration sinks in importance. In the Coal period we know few land animals, and these not vegetable feeders, with the exception of some insects, millepedes, and snails. But the Carboniferous forests did not live in vain, if their only use was to store up the light and heat of those old summers in the form of coal, and to remove the excess of carbonic acid from the atmosphere. In the Devonian period even these utilities fail, for coal does not seem to have been accumulated to any great extent, and the petroleum of the Devonian appears to have been produced from marine vegetation. A few insects are the only known tenants of the Devonian land, and these are of kinds whose larvæ probably lived in water, and were not dependent on land plants. We may have much yet to learn of the animal life of the Devonian; but for the present the great plan of vegetable nature goes beyond our measures of utility; and there remains only what is perhaps the most wonderful and suggestive correlation of all, namely, that our minds are able to trace in these perished organisms structures similar to those of modern plants, and thus to reproduce in imagination the forms and habits of growth of living things which so long preceded us on the earth.

[J. W. D.]

WEEKLY EVENING MEETING.

Friday, June 3, 1870.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Max Müller, M.A. LL.D.
PROFESSOR OF COMPARATIVE PHILOLOGY, OXFORD,

*On the Migration of Fables.**

Professor Max Müller began his discourse by tracing the proverb, "Count not your chickens before they be hatched," back to La Fontaine's fable, "La Laitière et le Pot au Lait," in which Perrette, carrying a milk-pail on her head, indulges in day-dreams; successively buying chickens, a pig, and a calf, from the profits of the sale of her milk, but losing all by the fall of her pail, through her imitating the kicking of the calf. This fable was first published, in 1678, in the second volume of La Fontaine's fables, and in the preface to that volume the poet states that he owes the largest portion of the subjects of his new fables to Pilpay, the Indian sage. The Professor then showed how rich the ancient literature of India was in fables, and how important a part they occupied in the sacred literature of Buddhism. In the 'Pañkatantra' (literally Pentateuch), a collection of Sanskrit fables, a story occurs of a Brahman who had saved a quantity of rice, which he kept in a jar suspended on a peg in the wall. Lying on a couch he began to dream how he would sell the rice and buy goats, sell them and buy cows, sell them and buy buffaloes, sell them and buy horses, and so on, till he had built a house and married a wife. He saw in his dream how his son is playing about; and as his wife allows the child to come too near the horse's hoofs, he gets very angry and trys to kick her. While he dreams, he actually kicks, and the jar of rice is smashed to pieces. The collection of fables in which this story occurs was translated from Sanskrit into Pehlevi (ancient Persian) in the time of Nushirvan, the King of Persia, A.D. 531–579, and afterwards translated from Persian into Arabic by Abdallah-ibn-Almokaffa, who lived at the court of the Chalif Almansur, 754–75. This Arabic version became the source of numerous translations into Greek, Hebrew, modern Persian, Latin, Spanish, and other languages which spread over Europe during the Middle Ages, and made most of the fables of the 'Pañkatantra,' familiar as household words in the literary centres of the West. After reading other forms of this fable, the Professor stated that the

* This discourse is printed *in extenso* in the 'Contemporary Review' for July, 1870.

change of the Brahman with his pot of rice into the milkmaid with her pot of milk took place in the thirteenth century, and was that adopted by La Fontaine. He then described more minutely the ramifications of this cluster of Indian fables in their progress from East to West, pointing out some other channels through which such tales as the stories of Sindbad reached the literary market of Europe. In conclusion, he showed that even a Father of the Church had exercised an important influence in importing Eastern fables into Europe. Joannes Damascenus lived at the court of the same Chalif Almansur for whom Abdallah-ibn-Almokaffa had translated the fables from Pehlevi into Arabic. In a religious novel, called 'Barlaam and Josaphat,' Joannes not only introduced a number of Eastern fables, but actually took his principal hero, Josaphat, from the 'Lalita-Vistara,' the life of the Buddha, or "Enlightened," a portion of the sacred canon of the Buddhists. That the story of Barlaam is, in its most striking points, a mere repetition of the story of the Buddha the Professor proved by quoting many names; and, finally, he drew attention to the fact that Josaphat, the hero of the story of Joannes Damascenus, had been raised to the rank of a saint both in the Eastern and Western Churches. "What follows, then?" the Professor asked. "It follows that Buddha has become a saint in the Romish Church; it follows that, though under a different name, the sage of Kapilavastu, the founder of a religion which, in the purity of its morals, is nearer to Christianity than any other religion, and counts even now, after an existence of 2400 years, 455,000,000 of believers, has received the highest honours which the Christian Church can bestow. And whatever we may think of the sanctity of saints, let those who doubt the right of Buddha to a place among the saints read the story of his life, as told in the Buddhist canon. If he lived the life which is there described, few saints have a better claim to that title than Buddha; and no one, either in the Greek or in the Roman Church, need be ashamed of having paid to his memory the honour which was intended for St. Josaphat, the Prince, the hermit, and the saint." [M. M.]

GENERAL MONTHLY MEETING,

Monday, June 6, 1870.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Miss Conway Griffiths.
Hugh Martin Macpherson, Esq. F.R.C.S.
William O. Priestley, M.D. F.R.C.P.
Henry Sutherland, Esq. M.B.

were *elected* Members of the Royal Institution.

The special thanks of the Members were returned for the following Donations to "the Fund for the Promotion of Experimental Researches":—

T. Williams Helps, Esq. (5th Donation)	£10
Erasmus A. Darwin, Esq. (2nd Donation)	25
Sir Henry Holland, Bart. M.D. (12th Annual Donation)	40

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Count of Paris and the Duke of Chartres—Campagnes de l'Armée d'Afrique 1835–39, par le Duc d'Orléans, publié par ses Fils. 8vo. Paris. 1870.
Anonymous—La Légitimité en Espagne: par H. de Lazeu. (K 97) 8vo. 1870.
Asiatic Society of Bengal—Journal, No. 158. 8vo.
Proceedings, 1870, No. 3. 8vo.
British Architects, Royal Institute of—Sessional Papers, 1869–70, No. 9. 4to.
British Museum Trustees—Catalogues of Sea Pens. 8vo. 1870.
Chemical Society—Journal for May, 1870. 8vo.
Editors—Academy for May, 1870. 4to.
Artizan for May, 1870. 4to.
Athenæum for May, 1870. 4to.
Chemical News for May, 1870. 4to.
Engineer for May, 1870. fol.
Horological Journal for May, 1870. 8vo.
Journal of Gas-Lighting for May, 1870. 4to.
Mechanics' Magazine for May, 1870. 8vo.
Nature for May, 1870. 4to.
Pharmaceutical Journal for May, 1870. 8vo.
Photographic News for May, 1870. 4to.
Revue des Cours Scientifiques et Littéraires. May, 1870. 4to.
Scientific Opinion. May, 1870. 4to.
Jablonowski'schen Society, Leipsic—Preisschriften, XV. With Plates. 4to. 1870.
Linnean Society—Journal, Nos. 48, 53. 8vo. 1870.
Meteorological Society—Proceedings, No. 49. 8vo. 1870.
North of England Institute of Mining Engineers—Transactions. Vols. XVII. XVIII. 8vo. 1867–9.
Photographic Society—Journal. No. 217. 8vo. 1870.
Prussian Academy, Royal—Monatsberichte, März, April, 1870. 1870. 8vo.
Royal Irish Academy—Transactions: Science, Parts 9—15; Polite Literature, Part 4; Antiquities, Part 8. 4to. 1869–70.
Royal Society of London—Proceedings, No. 119. 8vo. 1870.
Samuel, Messrs.—Wool and Woollen Manufactures of Great Britain. 8vo. 1859.
Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, May, 1870. 8vo.
Saxon Society of Sciences, Royal, Leipsic—Abhandlungen. 3 Parts. 4to. 1869.
Berichte: 1867. Nos. 3, 4. 1868. 1869. No. 1. 8vo.
T. Twining, Esq. M.R.I. (the Author)—Science for the People; a Memorandum. 8vo. 1870.
United Service Institution, Royal—Journal, No. 57. 8vo. 1870.
Van der Menbrugghe, C. (the Author)—Sur la Viscosité superficielle des Lames de la Solution de Saponine. 8vo. 1870.
Zoological Society—Transactions, Vol. VII. Parts 1, 2. 4to. 1870.
Proceedings for 1869. Parts 2, 3. 8vo.

WEEKLY EVENING MEETING,

Friday, June 10, 1870.

SIR H. HOLLAND, Bt. M.D. D.C.L. F.R.S. President, in the Chair.

WILLIAM ODLING, M.B. F.R.S.
FULLERIAN PROFESSOR OF CHEMISTRY, ROYAL INSTITUTION,

On the Ammonia Compounds of Platinum.

FOR nearly a century past ammonia gas, discovered by Priestley in 1774, has been a subject of extreme interest to chemists. This ammonia gas, H_3N, is especially characterized by its property of uniting directly with hydrochloric acid gas, HCl, to form a solid deposit of sal-ammoniac, or hydrochloride of ammonia, $H_3N.HCl$.

In several important particulars, sal-ammoniac presents a remarkable similarity of behaviour to chloride of potassium; and, by linking together the hydrogen of its acid with its ammonia so as to form the grouping $H_3N.H$ or H_4N, it may be regarded as the chloride of a composite metal ammonium, just as potassium chloride is the chloride of the simple metal potassium; thus :—

$(H_3N.H)Cl$ $\quad$ KCl

Ordinarily, when potassium chloride is subjected to the action of a weak current, no potassium, but only potash, makes its appearance at the negative pole; but if the negative pole be constituted of a drop of mercury, the electrolytically-liberated potassium remains dissolved in the mercury as potassium-amalgam K_xHg_y. Similarly, when solution of sal-ammoniac is subjected to electrolysis, the negative pole being constituted of mercury, there is produced a bulky amalgam of ammonium $(H_3N.H)_xHg_y$; which, however, when no longer under the influence of the current, speedily breaks up into ammonia, hydrogen, and mercury. Ammonium-amalgam may further be produced on a large scale by the action of potassium-amalgam or sodium-amalgam on sal-ammoniac solution, thus :—

$$x(H_3N.H)Cl + K_xHg_y = xKCl + (H_3N.H)_xHg_y$$

Another characteristic property of ammonia gas is its extreme solubility in water. By its dissolution it furnishes a liquid having many of the properties of aqueous potash, as, for example, the properties of affecting test-paper, of neutralizing acids, and of precipitating metallic salts. And just as sal-ammoniac may be regarded as a chloride of ammonium, analogous to chloride of potassium, so may aqueous ammonia be regarded as a hydrate of ammonium analogous to hydrate of potassium, thus :—

$(H_3N.H)HO$ $\quad$ KHO

But whereas chloride of ammonium, analogous to chloride of potassium, constitutes a definite body,—hydrate of ammonium, analogous to hydrate of potassium, has an inferential existence only. It is

inferred to exist in solution from the reactions of the solution; but, under all attempts at extraction, it breaks up into ammonia gas and water, thus:—

$$(H_3N.H)HO \text{ or } H_3N.H_2O = H_3N + H_2O$$

But by far the most interesting circumstance with regard to ammonia is its property, so remarkably developed by Hofmann, of serving as a type from which compounds of the most varied character are derivable by substitution. Just, for example, as the hydrocarbon residue, or radical, ethyl C_2H_5, can replace the hydrogen of hydrochloric acid to form ethylic chloride $C_2H_5.Cl$, so can it also replace the hydrogen of ammonia to form ethylamine $C_2H_5.H_2N$. This ethylamine constitutes a very volatile liquid, vaporizing considerably even at ordinary temperatures. Its vapour closely resembles ammonia gas, but is distinguishable therefrom by its ready inflammability. Like ammonia, ethylamine combines directly with hydrochloric acid to form ethylamine sal-ammoniac, or hydrochloride of ethylamine $C_2H_5H_2N.HCl$. Like ammonia, also, ethylamine is extremely soluble in water; and its solution, like that of ammonia, behaves in many respects as a definite hydrate $C_2H_5H_2N.H_2O$, not obtainable, however, in the isolated state, but, like hydrate of ammonia, known only in the state of solution.

There exist, moreover, derivatives of ammonia in which a portion of its hydrogen is replaced, not by a monad, but by a diad residue or radical. Just, for example, as diad ethylene, C_2H_4, replaces the hydrogen of two units of hydrochloric acid to form ethylenic chloride, $C_2H_4.Cl_2$, so can it also replace, in part, the hydrogen of two units of ammonia to form ethylenamine, $C_2H_4(H_2N)_2$ or $(C_2H_4)''H_4N_2$. This double ammonia unites with *two* units of hydrochloric acid to form the definite hydrochloride, $C_2H_4(H_2N)_2.2HCl$, and with *two* units of water to form the equally definite, stable, isolable, volatile, crystallizable hydrate $C_2H_4(H_2N)_2.2H_2O$.

The hydrochloride and non-isolable hydrate of ethylamine being compared with the chloride and hydrate of the monad alkali-metal potassium, the hydrochloride and isolable hydrate of ethylenamine are similarly comparable with the chloride and hydrate of the diad alkali-metal barium, thus:—

$K'Cl$	$\{C_2H_5(H_2N)H\}'Cl$	$Ba''Cl_2$	$\{C_2H_4(H_2N)_2\}''Cl_2$
$K'HO$	$\{C_2H_5(H_2N)H\}'HO$	$Ba''(HO)_2$	$\{C_2H_4(H_2N)_2\}''(HO)_2$

And hydrate of ethylenamine agrees with hydrate of barium, as well in being a powerfully alkaline base, as in being de-hydrateable, not by the action of heat, but by indirect methods only.

Through the further researches of Hofmann, chemists are acquainted with ammonias and di-ammonias, in which, not only one-third, but two-thirds and three-thirds of the hydrogen are replaced by monad ethyl and diad ethylene respectively. With regard to these compounds, both in their properties and in the nature of the

hydrochlorides and hydrates which they furnish, di- and tri-ethylamine correspond very closely to ethylamine—di- and tri-ethylenamine very closely to ethylenamine.

	Ethylamines.	Ethylenamines.
Mono-	$(C_2H_5)'H_2N$ ..	$(C_2H_4)''H_4N_2$
Di-	$(C_2H_5)'_2HN$ ..	$(C_2H_4)''_2H_2N_2$
Tri-	$(C_2H_5)'_3N$ ..	$(C_2H_4)''_3N_2$

Although, in this way, the principal developments and ultimate establishment of the idea of ammonia as a type have resulted from investigations in organic chemistry, the idea itself appears to have originated in the first instance from investigations made in mineral chemistry; and especially from the investigation of compounds formed by the reaction of certain metallic salts and ammonia. Graham, indeed, had early represented certain compounds of metallic chlorides with ammonia as being metallicized sal-ammoniacs; but the notion of ammonia as a trihydric type, susceptible of three successive degrees of substitution, was first enunciated by Laurent, and was employed by that most original chemist to explain, among other matters, the constitution of different ammoniated compounds of platinum, discovered by Magnus, Gros, and Reiset successively. These compounds he represented as being salts of derived ammonias, in which different proportions of the hydrogen of ammonia were replaced by platinum.

Platinum is a moderately hard, pewter-coloured metal, possessed of many singular properties. It was first recognized as a distinct metal by Wood, an assayer of Jamaica, in 1741. The mode of working it was discovered and practised by Wollaston early in the present century, and described by him in the 'Philosophical Transactions' for 1829. Platinum is especially characterized by its high specific gravity; by its low conductivity, dilateability, and specific heat; by its high ductility and tenacity; by its facile divisibility and reducibility; by its curious absorptivity of certain gases, more especially hydrogen; by its difficult attackability by chemical agents; and by its infusibility at the highest furnace heats. And by each of these several properties, except perhaps its high specific gravity, is it suited to some special application in the arts.

Chemically, platinum is characterized by its high atomic weight, 197; and by its formation of two well-defined chlorides,—a perchloride, also known as platinic chloride, expressed by the formula $Pt''''Cl_4$, and a protochloride, also known as platinous chloride, expressed by the formula $Pt''Cl_2$. Platinic chloride occurs in crystalline, dark orange masses, freely soluble in water. Platinous chloride forms an olive-brown amorphous powder, quite insoluble in water, but dissolving in hydrochloric acid to form an ochre-coloured liquid. In 1828, the late Professor Magnus, by supersaturating this liquid with ammonia, obtained a remarkable compound, containing the elements of platinous chloride and ammonia, and presenting itself as a dull green, usually crystalline, precipitate. This notable green precipitate has formed the subject of

frequent investigation from then till now; and different views of its constitution, from time to time, have been put forward. But no one of these views has received a sufficiently general acceptance to warrant the designation of the compound in accordance therewith; so that, from the period of its discovery down to the present day, it has ever borne the honoured name of its discoverer, and been known as the green salt of Magnus.

Some time after, Gros, in 1838, by treating the salt of Magnus with nitric acid, obtained a peculiar series of pale yellow, or colourless, platin-ammonia compounds. Next, in 1840–44 *et seq.*, Reiset and Peyrone, by acting on the salt of Magnus, or on platinous chloride itself, with ammonia, obtained, independently of each other, two additional series of compounds, having relations of metamorphosis both with one another and with the foregoing series of Gros. Then, in 1846, Raewsky, by acting on the salt of Magnus with nitric acid, obtained yet another series of compounds, differing from those furnished to Gros by the same reagent.

But the formulæ, attributed by Raewsky to the salts which he had just discovered, were inconsistent both with Laurent's view of ammonia as a type, and with other views of chemical constitution which those illustrious fellow-workers Laurent and Gerhardt shared in common. Accordingly, Gerhardt, in 1848, subjected Raewsky's formulæ,—notwithstanding their corroboration by a Committee of the Academy,—to a most trenchant criticism; and after supplementing his criticism by laboratory research, published in 1850, his celebrated memoir "On the Ammoniacal Compounds of Platinum." In this memoir he established the existence of another, entirely new, series of platin-ammonia compounds; he showed, by experiment, the simple relationship in which the salts of Raewsky stand to the salts of Gros; and he set forth a complete, self-consistent scheme of viewing the several series of platin-ammonia compounds in their relations to ammonia and to one another.

Since the publication of Gerhardt's memoir, further important contributions to the knowledge of platin-ammonia compounds have been made by different chemists, especially by Buckton, Hadow, and Thomsen. But neither before nor since has any complete general scheme of the constitution of this class of bodies been put forward. Nevertheless the scheme of Gerhardt, though always treated with respect, has never met with general acceptation, and nowadays, at any rate, is open to very serious objections.*

The attention of the writer having of late been directed to the study of these compounds, he has succeeded in differentiating the simplest of the platinum sal-ammoniacs from several allied and isomeric bodies with which it had before been confounded; and in obtain-

* Gerhardt's own base platinamine, for instance, is represented by the certainly improbable formula pt_2HN or $Pt''HN$, and its hydrochloride by the yet more improbable formula $pt_2HN.2HCl$; in which the unit of a mon-ammonia is represented as combined with two units of hydrochloric acid, and by which the entire chlorine of the salt is represented as fulfilling one and the same function.

ing from it the corresponding hydrated base of the series. He has also obtained some reactions of interest with bodies belonging to the more complex series: and as a general result of his inquiries, has ventured to put forward a new scheme of regarding the entire group of bodies. This scheme is based on the recognition of two principal facts or propositions:—

1°. The different platin-ammonia compounds are produced in the first instance from platinous chloride $PtCl_2$: and just as the platinum of this compound possesses the property of taking up two additional proportions of chlorine so as to furnish the platinic compound Cl_2PtCl_2 or $Pt^{\prime\prime\prime\prime}Cl_4$, so also does the platinum of the different ammoniated bodies obtained from platinous chloride possess the property of taking up two proportions of chlorine, or its equivalent of other negative radical, so as to furnish platinic compounds corresponding to the original platinous compounds respectively. Hence the division of platin-ammonia compounds into the two classes of platinous and platinic: the compounds of the former, differing in constitution from those of the latter class, just as platinic differs from platinous chloride, by a direct fixation of chlorine.

2°. The monad residue or radical amidogen H_2N, is capable of becoming the monad radical ammon-amidogen $H_2N.H_2N$ or H_4N_2, just as the monad radical methyl H_3C, is capable of becoming the monad radical methylen-methyl or ethyl $H_3C.H_2C$ or H_5C_2.* Viewing sal-ammoniac $H_4N.HCl$ as the analogue of methylic chloride $H_3C.HCl$, the difference is noticeable, that while the ammonia both of sal-ammoniac and ammon-amidogen is easily separable, the methylen both of methylic chloride and methylen-methyl is inseparable from the remaining constituents of the respective compounds. Hence the distinction between the two classes of amic and ammon-amic platinum compounds, the latter differing from the former by an actual addition of diad ammonia, much as ethylic differ from methylic compounds by a virtual addition of diad methylen. The parallelism is indicated in the under-written formulæ for methylic chloride and ethylic chloride, sal-ammoniac and ammonio-chloride of silver, respectively:—

$H_3C.HCl$	$H_3C\left\{\begin{matrix}H\\Cl\end{matrix}\right.$	H_4N_2 HCl	$H_2N\left\{\begin{matrix}Ag\\H_2N\\Cl\end{matrix}\right.$

The group of platin-ammonia compounds is thus divisible into the two classes of platinous and platinic; and each of these again into the two classes of amic and ammon-amic compounds. To these four classes must yet be added a fifth sub-class of di-platinic compounds, including the nitrate-chloride of Raewsky and the subsequently-discovered nitrate of Gerhardt and chloride of Hadow. The scheme of the constitution of the entire group, in accordance with the writer's views, is exhibited in the accompanying table of the principal chloride, hydrate, nitrate and nitrite compounds.

* Phil. Mag. 1863, p. 488.

PLATINUM BASES AND SALTS.

PROPOSED SCHEME.

Platosamine.	*Amo-platosamine* (Reiset's).
$Pt''(H_2N)_2 . 2HCl$	$Pt''(H_5N_2)_2 . 2HCl . Aq.$
$Pt''(H_2N)_2 . 2H(HO)$	$Pt''(H_5N_2)_2 . 2H(HO)$
$Pt''(H_2N)_2 . 2H(NO_3)$	$Pt''(H_5N_2)_2 . 2H(NO_3)$
Platinamine (Gerhardt's).	*Amo-platinamine* (Gros').
$Cl_2Pt''''(H_2N)_2 . 2HCl$	$Cl_2Pt''''(H_5N_2)_2 . 2HCl$
$Cl_2Pt''''(H_2N)_2 . 2H(HO)$?	$Cl_2Pt''''(H_5N_2)_2 . 2H(NO_3)$
$(HO)_2Pt''''(H_2N)_2 . 2H(HO)$	$(HO)_2Pt''''(H_5N_2)_2 . 2H(NO_3)$
$OPt''''(H_2N)_2 . 2H(NO_3) . 3Aq.$?	$(NO_3)_2Pt''''(H_5N_2)_2 . 2H(NO_3)$
$(NO_3)_2Pt''''(H_2N)_2 . 2H(NO_3)$	$(NO_3)_2Pt''''(H_5N_2)_2 . 2HCl$

Amo-diplatinamine (Raewsky's, &c.).

$Cl_2O\,Pt_2''''(H_5N_2)_4 . 4HCl$

$Cl_2O\,Pt_2''''(H_5N_2)_4 . 4HNO_3 . Aq.$

$(NO_3)_2O\,Pt_2''''(H_5N_2)_4 . 4HNO_3 . Aq.$

Platosamine.

The hydrochloride is obtainable by direct action of platinous chloride $Pt''Cl_2$, on ammonia, just as ethylenamine hydrochloride is obtainable by direct action of ethylenic chloride $(C_2H_4)''Cl_2$, on ammonia; and, in both reactions, the two compounds are each accompanied by various other products. The parallelism, in constitution and properties, of the two compounds and of their corresponding hydrates is complete:—

$Pt''(H_2N)_2 . 2HCl$	$C_2H_4(H_2N)_2 . 2HCl$
$Pt''(H_2N)_2 . 2H_2O$	$C_2H_4(H_2N)_2 . 2H_2O$

Hydrochloride of platosamine, in its pure state, has a dull, very pale, primrose colour, approaching almost to white. It is very insoluble in cold, considerably more soluble in hot water.* From its concentrated hot solution it is usually deposited as a flocculent-looking, though really crystalline, or partly crystalline, precipitate; and by slow cooling of the solution, it may be obtained wholly in the form of distinct needles. The base is obtained from the hydrochloride, through the intervention of the sulphate, by decomposing the latter with baryta water. It is extremely soluble in water, and readily crystallizable. It furnishes a solution manifesting a strongly alkaline reaction, liberating ammonia from its salts, neutralizing acids, absorbing carbonic acid from the air, and decomposing metallic salts—the precipitates thrown down being, however, for the most part double compounds.

* The orange-yellow, scaly, far more soluble hydrochloride, obtained by dissolving salt of Magnus in sulphate of ammonia solution, is a distinct compound.

The base and other soluble platosamine compounds are characterized by giving with hydrochloric acid a precipitate of the hydrochloride, which, when formed in this way, is almost always yellow and crystalline. All platosamine compounds, including the hydrochloride, exhibit the platinous property of absorbing chlorine, and hence of decolourizing a mixture of hydrochloric acid and permanganate, to yield platinamine compounds; and they all, including the hydrochloride, dissolve in gently-heated ammonia, to yield their corresponding amo-platosamine compounds.

Amo-platosamine.

The hydrochloride of this series, unlike that of the preceding, is freely soluble in water. It is a beautiful salt, crystallizing usually in a mass of interlaced lengthy needles. It is made by dissolving platinous chloride, salt of Magnus, or the above platosamine hydrochloride, in aqueous ammonia at a gentle heat. The hydrated base is obtained from it, as is the preceding hydrated base from the preceding hydrochloride. The so-produced hydrate corresponds in its general properties with the above hydrate of platosamine, than which, however, it is far more powerfully alkaline. From different metallic salts it throws down the respective metallic hydrates.

Amo-platosamine salts, like those of platosamine, exhibit the platinous property of decolourizing a mixture of hydrochloric acid and permanganate, of directly absorbing chlorine or bromine, and also of directly absorbing nitric peroxide, to furnish amo-platinic compounds.* The resulting amo-platinic chloro-chloride occurs as a pale yellow, the nitro-chloride as a pale green, and the nitro-nitrate as a pale blue precipitate. Amo-platosamine is further distinguished from platosamine by the free solubility of its hydrochloride, and by the reaction of this salt with platinous chloride solution to throw down the green salt of Magnus.

Platinic Compounds.

The most characteristic of the salts of platinamine is the chloride $Cl_2Pt''''(H_2N)_2.2HCl$. It is best made by addition of permanganate, in very slight excess, to a hot solution of platosamine hydrochloride, acidified with hydrochloric acid. It is a beautiful bright yellow salt, dissolving sparingly in cold, moderately, though, on account of its density, somewhat slowly, in boiling water; and crystallizing readily on cooling in isolated octahedrons or square plates. It reacts with excess of ammonia, at a gentle heat, to form the insoluble chloride of amo-platinamine. Its chlorine, like that of the amo-platinic chloride, is evidently in two different conditions of attackability by reagents, such as alkalies and silver salts. The hydrate of platinamine is ob-

* The reaction of amo-platosamine compounds with nitric peroxide was discovered by Hadow, but received from him a different interpretation.

tained from the hydrate-nitrate, by means of ammonia, as a neutral, almost insoluble, bright yellow, crystalline precipitate.

The best known salt of amo-platinamine is the chloro-nitrate $Cl_2Pt''''(N_2H_2)_2 . 2HNO_3$, obtained by treating the salt of Magnus, or preferably the hydrochloride of amo-platosamine, with nitric acid. It is moderately soluble in water, and crystallizes therefrom in brilliant white flat prisms. Its chlorine is not immediately recognizable by nitrate of silver; and is only partially precipitable thereby, even after long boiling. Its solution yields, with ammonium chloride and sodium sulphate, crystalline white precipitates of the chloro-chloride and chloro-sulphate respectively.

The most familiar salt of amo-diplatinamine is the chloroxy-nitrate $Cl_2OPt_2''''(H_2N_2)_4 . 4HNO_3 . Aq$. It is best made by boiling the chloro-nitrate or chloro-chloride of amo-platinamine with nitric acid and nitrate of silver. It presents considerable resemblance to the chloro-nitrate of amo-platinamine, but its solution is not disturbed either by sulphate of sodium or chloride of ammonium. It yields moreover, with platinous chloride solution, a moss-like coppery precipitate which is highly characteristic (Hadow).

[W. O.]

GENERAL MONTHLY MEETING,

Monday, July 4, 1870.

Sir Roderick I. Murchison, Bart. K.C.B. D.C.L. F.R.S.
Vice-President, in the Chair.

Arthur Balfour, Esq.
James Davenport, M.D.
James Baumgardt Elmslie, Esq.
Captain Herbert P. De Kantzow, R.N.

were *elected* Members of the Royal Institution.

The Secretary announced the receipt of a Legacy of £2000 from the late Alfred Davis, Esq. M.R.I. for the Promotion of Philosophical Researches.

Resolved, That the Members of the Royal Institution cannot hear of the munificence of their late fellow-member, Mr. Alfred Davis, without expressing their admiration of the liberality of one, who not only in his life gave large gifts to help the good works of Jews and Christians, but by his will has continued his unrestricted benefactions by many noble legacies.

The Members have on various occasions given their special thanks to Mr. Alfred Davis for his Donations to the Fund for the Promotion of Scientific Research, and the application of the interest of his legacy to the same good object will yearly confer a fresh benefit upon the Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Académie Royale de Belgique—Bulletins, 2e série, Tomes XXVII. XXVIII. 8vo. 1869. Almanach, 1870. 16mo.

Astronomical Society, Royal—Monthly Notices, Vol. XX. Nos. 7, 8. 8vo. 1870.

Bavarian Academy of Sciences, Royal—Sitzungsberichte, 1869. Band II. Heft 3, 4. 1870. Heft 1. 8vo.

Beauclerc, Major G. M.R.I. (the Author)—Observations on the Authenticity of a Portrait of Mary Queen of Scots. (K 98) 8vo. 1870.

Chemical Society—Journal for June, 1870. 8vo.

Comitato Geologico d'Italia—Bollettini, Nos. 4, 5. 8vo. Firenze, 1870.

Editors—Academy for June, 1870. 4to.
American Journal of Science, May, 1870. 8vo.
Artizan for June, 1870. 4to.
Athenæum for June, 1870. 4to.
Chemical News for June, 1870. 4to.
Engineering for June, 1870. fol.
Horological Journal for June, 1870. 8vo.
Journal of Gas-Lighting for June, 1870. 4to.
Mechanics' Magazine for June, 1870. 8vo.
Nature for June, 1870. 4to.
Pharmaceutical Journal for June, 1870. 8vo.
Photographic News for June, 1870. 4to.
Revue des Cours Scientifiques et Littéraires. June, 1870. 4to.
Scientific Opinion. June, 1870. 4to.

Franklin Institute—Journal, Nos. 531, 532. 8vo. 1869.

Geographical Society, Royal—Proceedings, 1870, No. 2. 8vo.

Geological Institute, Imperial, Vienna—Jahrbuch, 1870, No. 1. 8vo.
Verhandlungen, 1870, No. 1. 8vo.

Institut Impérial de France, Académie des Sciences—Mémoires, Tome XXXVI. Tome XXXVII. 2e Partie. 4to. 1870.

Jones, H. Bence, M.D. F.R.S. Sec. R.I.—Report on Cattle Plague in Great Britain, 1865, 1866, and 1867. fol. 1868.

Linnean Society—Proceedings, 1869–70. 8vo.

Rogers, N. M.D. (the Author)—Bibliography. (K 91) 8vo. 1870.

Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, June, 1870. 8vo.

Tyndall, Professor, LL.D. F.R.S. (the Author)—Researches on Diamagnetism and Magne-Crystallic Action. 8vo. 1870.
Notes on Light. 8vo. 1870.

Royal Institution of Great Britain.

GENERAL MONTHLY MEETING,

Monday, Nov. 7, 1870.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

John Forbes, Esq. M.R.C.P. F.R.C.S.
Thomas Miller Rickman, Esq. F.S.A.
The Rev. Arthur Rigg, M.A.
Thomas Taplin, Esq.

were *elected* Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

Bequeathed by the late Sir James South.

MS. Notes by Mr. Faraday. 5 vols. 4to.
Notes of Lectures by Mr. Tatum at the City Philosophical Society. 1810–11. 4 vols.
Notes of Lectures by Sir Humphry Davy. 1 vol. 1812.
Mr. Faraday's Iron Ring, from which he obtained the first Magneto-electric Spark.

Presented by Dr. A. J. Stephens, Q.C., Sir James South's Executor.

Mr. Faraday's First Electric Machine.

FROM

Commissioners in Lunacy—24th Report. 8vo. 1870.

Académie Impériale des Sciences de St. Pétersbourg—Mémoires. VII^e^ Série, Tome XIV. Nos. 8, 9. Tome XV. Nos. 1–8. 4to. 1870.
Bulletins, Tome XIV. Nos. 4, 5, 6. Tome XV. Nos. 1, 2. 4to. 1870.
Jahresbericht des Physikalischen Central-Observatoriums für 1869. 4to. 1870.
Actuaries, Institute of—Journal, No. 81. 8vo. 1870.
Agricultural Society of England, Royal—Journal, Second Series, No. 12. 8vo. 1870.
American Academy of Arts and Sciences—Proceedings, Vol. VIII. Nos. 1–17. 8vo. 1868–9.
American Philosophical Society—Proceedings, No. 82. 8vo. 1869.
American Academy of Natural Sciences—Proceedings, 1869. 8vo.
American Association for the Advancement of Science—Proceedings, 1868. 8vo. 1869.
Anonymous—Ambition's Dream. A Poem. (O 13) 16mo. 1870.

Antiquaries, Society of—Archæologia, Vol. XLII. No. 2. 4to. 1870.
Proceedings, Vol. IV. No. 8. 8vo. 1870.
Asiatic Society of Bengal—Journal, Nos. 160, 161, 162. 8vo. 1870.
Proceedings, 1870, Nos. 4, 5, 6, 7. 8vo.
Bavarian Academy of Sciences, Royal—Sitzungsberichte. 1870. Hefte 2, 3. 8vo.
Bombay Geographical Society—Transactions Vol. XIX. Part 1. 8vo. 1870.
Boston Society of Natural History, U.S.—Proceedings, Vol. XII. Nos. 18–25. Vol. XIII. Nos. 1–14. 8vo. 1869.
A. Goul l, Invertebrata of Massachussetts. 8vo. 1870.
British Architects, Royal Institute of—Sessional Papers, 1869–70. No. 13. 4to.
British Museum Trustees—Catalogue of Fishes, Vol. VIII. 8vo. 1870.
Guides to Autograph Letters, Minerals, and Exhibition Rooms. 8vo. 1870.
Catalogue of Lithophytes. 8vo. 1870.
Supplement to Catalogue of Shield Reptiles. 4to. 1870.
Chemical Society—Journal for July–Oct. 1870. 8vo.
Clinical Society—Transactions, Vol. III. 1870.
Comitato Geologico d'Italia—Bollettino, No. 6. 8vo. Firenze, 1870.
Davis, Sir John F. Bart. K.C.B. F.R.S. (the Author)—The Poetry of the Chinese. 4to. 1870.
Editors—American Journal of Science, July, Aug. 1870. 8vo.
Artizan for July to Oct. 1870. 4to.
Athenæum for July to Oct. 1870. 4to.
Chemical News for July to Oct. 1870. 4to.
Engineer for July to Oct. 1870. fol.
Food Journal for Aug. 1870.
Horological Journal for July to Oct. 1870. 8vo.
Journal of Gas-Lighting for July to Oct. 1870. 4to.
Quarterly Journal of Science for Oct. 1870. 8vo.
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GENERAL MONTHLY MEETING,

Monday, Dec. 5, 1870.

WILLIAM SPOTTISWOODE, Esq. M.A. F.R.S. Treas. R.S. Treasurer and Vice-President, in the Chair.

John Colebrook, Esq. M.R.C.S.
Lieut. William Cecil Henry Domville, R.N.
Dyce Duckworth, M.D. F.R.C.P.
Miss Millicent Bence Jones.
William Berkeley Monck, Esq.
Thomas Sydney Smyth, Esq.
Rev. Charles Frederick Stovin.

were *elected* Members of the Royal Institution.

Lecture Arrangements for the ensuing Season :—

Prof. ODLING, M.B. F.R.S.—Six Lectures, adapted to a Juvenile Auditory, On Burning and Unburning, on December 27, 29, 31, 1870; January 3, 5, 7, 1871.

Before Easter, 1871.

Prof. MICHAEL FOSTER, M.D. F.L.S.—Eleven Lectures, On the Nutrition of Animals, on Tuesdays, January 17 to March 28.

Prof. ODLING, F.R.S.—Eleven Lectures, On Davy's Discoveries in Chemistry, on Thursdays, January 19 to March 30.

Rev. W. H. CHANNING, M.A.—Four Lectures, On the Laws of Human Life as revealed in History, on Saturdays, January 21 to February 11.

Prof. JOWETT, M.A. Master of Balliol College, Oxford.—Three Lectures, On Socrates, on Saturdays, February 18, 25, and March 4.

H. O'NEIL, Esq. R.A.—Four Lectures, On the Spirit of the Age, on Saturdays, March 11 to April 1.

After Easter, 1871.

WILLIAM PENGELLY, Esq. F.R.S.—Three Lectures, On the Geology of Devonshire, and especially of the New Red Sandstone System, on Tuesdays, April 18, 25, and May 2.

CHARLES BROOKE, Esq. M.A. LL.D. F.R.S.—Two Lectures, On Force and Energy, on Tuesdays, May 9 and 16.

Rev. Prof. HAUGHTON, M.D. F.R.S.—Three Lectures, On the Principle of Least Action in Nature, illustrated by Animal Mechanics, on Tuesdays, May 23, 30, and June 6.

Prof. TYNDALL, LL.D. F.R.S.—Eight Lectures, On Sound, on Thursdays, April 20 to June 8.

JOSEPH NORMAN LOCKYER, Esq. F.R.S.—Eight Lectures, On the Instruments used in Modern Astronomy, on Saturdays, April 22 to June 10.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:

FROM

Bavarian Academy of Sciences, Royal—Sitzungsberichte. 1870. Heft. 4. 8vo.
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Engineer for Nov. 1870. fol.
Horological Journal for Nov. 1870. 8vo.
Journal of Gas-Lighting for Nov. 1870. 4to.
Mechanics' Magazine for Nov. 1870. 8vo.
Pharmaceutical Journal for Nov. 1870. 8vo.
Photographic News for Nov. 1870. 4to.
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Tyndall, Professor, LL.D. F.R.S. (the Author)—Essays on the Use and Limit of the Imagination in Science. 8vo. 1870.
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1871.

WEEKLY EVENING MEETING,

Friday, January 20, 1871.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

JOHN TYNDALL, Esq. LL.D. F.R.S.
PROFESSOR OF NATURAL PHILOSOPHY IN THE ROYAL INSTITUTION,

On the Colour of Water, and on the Scattering of Light in Water and in Air.

The Method of Research.

BY the term "scattering" employed in the title of this discourse I intend to denote the irregular reflexion of light from particles mechanically suspended in water or in air. Such particles, you know, reveal themselves along the track of a beam of light when it passes through a dark room. Such a beam, wide at some parts and powerfully concentrated at others, is now before you, the space it occupies being accurately revealed by the floating motes. We can abolish this scattering either by the withdrawal of the light or by the destruction of the floating matter. I partially intercept this beam; the space corresponding to the intercepted portion is dark, because there no light is scattered. I destroy the matter by burning it, and spaces of darkness immediately appear where a moment ago we had myriads of shining motes. These experiments are familiar to most of you. To see these effects aright, it is necessary that the eye should receive light from the floating dust alone, which is thus differentiated, as philosophers say, from the unilluminated space around it. This point is easily illustrated by means of the sun-burners which illuminate this room. When the gas is turned on the track of the beam sinks in brilliancy; when the gas is turned off the strength of the beam is augmented.

The Oran Eclipse Expedition.

During the last two or three years I have been looking much at illuminated dust, and from what I have heard or read of that halo or glory called the corona, which is seen around the sun during total eclipses, I thought it might be caused by a kind of solar dust, cast possibly into space by the enormous agitations and explosions to which

the sun's photosphere is known to be subject. Thoughts of this kind —and they were not the only ones—made me wish to see the corona, and I therefore availed myself of the permission courteously granted me to accompany the Eclipse Expedition to Oran. Every member of the expedition was, I believe, perfectly prepared through previous discipline to do his duty efficiently, and if the party was defeated it certainly was not through any misdeeds of its own.

From the roof of the bastionet on which I had placed the excellent instrument lent to me by my friend, Mr. Warren de la Rue, I witnessed a very striking effect, which connects itself with this subject of the scattering of light. When all hope had vanished, and the darkness was obviously falling towards totality, I abandoned the telescope and looked towards a distant ridge, where I knew the darkness would first appear. At this time a vast fan of beams, issuing from the hidden sun, covered all the southern sky. These beams, as you know, are alternations of light and shadow, produced by cloudlets of various degrees of density, floating in illuminated haze. The beams are really parallel, but by the effect of perspective they appear divergent, like a fan. The darkness took possession of the ridge to which I have referred, and immediately afterwards passed over the region of the beams, blotting them out as if a sponge had been passed over them. It then took possession of three successive patches of blue sky in the south-eastern heaven. I again looked towards the ridge where the darkness first appeared. A glimmer like a dawn was behind it, and immediately afterwards the fan of beams, which had been for two minutes obscured, revived in all its strength and splendour. The eclipse was ended, we had been defeated, and nothing remained but to strangle disappointment by taking up other work.

The Colours of Sea Water.

The colour of the sea had long interested me, and on my way out I collected a number of bottles of water, with a view to subsequent examination. But my bottles were claret bottles, and I could by no means feel sure of their purity. At Gibraltar, therefore, I purchased fifteen white glass bottles, with ground-glass stoppers, and at Cadiz, on my return, I secured a dozen more. These seven-and-twenty bottles were filled with water, taken at different places along the line between Algeria and Spithead.

And here it is most fit that I should express my acknowledgments to Captain Henderson, the commander of H.M.S. 'Urgent,' who aided me in my observations in every possible way. Indeed, my thanks are due to all the officers for their unfailing courtesy and help. The captain placed at my disposal his own coxswain, a handsome intelligent fellow named Thorogood, who skilfully attached a cord to each bottle, weighted it with lead, cast it into the sea, and after three successive rinsings, filled it under my own eyes. The contact of jugs, buckets, and other vessels, was thus avoided, and even the necessity of pouring the water out afterwards through the dirty London air.

The process of examination to which these bottles were subjected after my return to London is in some sense complementary to that of the microscope, and may, I think, materially aid inquiries conducted with that instrument. In microscopic examination attention is directed to a small portion of the liquid, and the aim is to detect the individual suspended particles. By the method I pursued, a large portion of the liquid is illuminated, its general condition being revealed, through the light scattered by suspended particles. Care is taken to defend the eye from the access of all other light, and thus defended, it becomes an organ of inconceivable delicacy. Were the water perfectly free from matter, in a state of mechanical suspension, it would, in my opinion, scatter no light at all. The track of a luminous beam could not, I think, be seen in such water. But an amount of impurity so infinitesimal as to be scarcely expressible in numbers, and the individual particles of which are so small as wholly to elude the microscope, may, when examined by the method alluded to, produce not only sensible but striking effects upon the eye.

For the sake of sparing time, let us direct our attention to these nineteen bottles, filled at various places between Gibraltar and Spithead. The results of the examination of the water in these nineteen bottles are tabulated before you. We have first three specimens of water, described as green, a clearer green, and bright green, taken in Gibraltar Harbour, at a point two miles from the harbour, and off Cabreta Point. What does the home examination reveal about these waters? It says that the first was thick with suspended matter, the second less thick, and the third still less thick. The green brightened as the suspended matter became less. We now pass suddenly into indigo water. What change does the home examination here reveal? The water becomes suddenly augmented in purity, for the suspended matter has become suddenly less. Off Tarifa, the deep indigo disappears, and the sea is undecided in colour. Accompanying this change, we have a rise in the quantity of suspended matter. Beyond Tarifa, we change to cobalt-blue, the suspended matter falling at the same time in quantity. This water is distinctly purer than the green. We approach Cadiz, and at twelve miles from the city get into yellow-green water; this the London examination shows to be thick with suspended matter. The same is true of Cadiz Harbour, and also of a point fourteen miles from Cadiz in the homeward direction. Here there is a sudden change from yellow-green to a bright emerald green, and accompanying the change a sudden fall in the quantity of suspended matter. Between Cape St. Mary and Cape St. Vincent the water changes to the deepest indigo. In point of purity, this indigo water is shown by home examination to transcend the emerald-green water.

We now reach the remarkable group of rocks called the Burlings, and find the water between the shore and the rocks a strong green; the home examination shows it to be thick with fine matter. Fifteen or twenty miles beyond the Burlings we come again into indigo water, from which the suspended matter has in great part disappeared. Off

Cape Finisterre, about the place where the 'Captain' went down, the water becomes green, and the home examination pronounces it to be thicker. Then we enter the Bay of Biscay, where the indigo resumes its powers, and where the home examination shows the greatly augmented purity of the water. A second specimen of water taken from the Bay of Biscay held in suspension fine particles of a peculiar kind; the size of them was such as to render the water richly iridescent. It showed itself green, blue, or salmon colour, according to the direction of the line of vision. Looked at obliquely *from* the light the water was blue. Finally, we come to our last two bottles, the one taken opposite St. Catherine's Lighthouse, in the Isle of Wight, the other at Spithead. The sea at both these places was green, and both specimens, as might be expected, were pronounced by the home examination to be thick with suspended matter.

Two distinct series of observations have been thus brought before you, the one consisting of direct observations of the colour of the sea, conducted during the voyage from Gibraltar to Portsmouth; the other conducted in our laboratory below stairs. And here it is to be noted that in the home examination I never knew what water I had in my hands. The labels, which had written upon them the names of the localities, had been tied up as you see them here, all information regarding the source of the water being thus precluded. The bottles were simply numbered, and not till all the waters had been examined did I open the labels, and ascertain the locality and sea colours corresponding to the various specimens. I must, therefore, have been perfectly unbiassed in my home observations, and they establish beyond a doubt the association of the green colour of sea water with fine suspended matter, and the association of the ultramarine colour, and more especially of the black indigo hue of sea water, with the comparative absence of such suspended matter.

The Causes of the Colours of the Sea.

Let us clear our way by a few experiments towards an explanation of the dark hue of the deep ocean.* Colour, you know, resides in white light, appearing generally when any constituent of the white light is withdrawn. Here is a liquid which colours a beam sent through it purple, and this colour is immediately accounted for by the action of the solution on a spectrum. It cuts out the yellow and green, and allows red and blue to pass through. The blending of these two colours produces the purple. Does the liquid allow absolutely free passage to the red and blue? No. It enfeebles the whole

* A note written to me the 22nd of October, by my friend Canon Kingsley, contains the following reference to this point:—"I have never seen the Lake of Geneva, but I thought of the brilliant, dazzling dark blue of the mid Atlantic under the sunlight, and its black blue under cloud, both so solid that one might leap off the sponson on to it without fear; this was to me the most wonderful thing which I saw on my voyage to and from the West Indies."—J. T.

spectrum, but attacks with special energy the yellow and green colours. By increasing the thickness of the stratum traversed by the beam, we cut off the whole of the spectrum. Through the deeper layer, which I now place in the path of the beam, no colour can pass. Here, again, is a blue liquid. Why is it blue? Its action on the spectrum answers the question. It first extinguishes the red; then as the thickness augments it attacks the orange, yellow, and green in succession; the blue alone finally remains, but everything might be extinguished by a sufficient depth of the liquid.

And now we are prepared for a concentrated but tolerably complete statement of the action of sea water upon light, to which it owes its blackness. Here is our spectrum. This embraces three classes of rays—the thermal, the visual, and the chemical. These divisions overlap each other; the thermal rays are in part visual, the visual rays in part chemical, and *vice versâ*. The vast body of thermal rays is here beyond the red and invisible. They are attacked with exceeding energy by water. They are absorbed close to the surface of the sea, and are the great agents in evaporation. At the same time the whole spectrum suffers enfeeblement; water attacks all its rays, but with different degrees of energy. Of the visual rays the red are attacked first, and first extinguished. While the red is extinguished, the remaining colours are enfeebled. As the solar beam plunges deeper into the sea, orange follows red, yellow follows orange, green follows yellow, and the various shades of blue, where the water is deep enough, follow green. Absolute extinction of the solar beam would be the consequence if the water were deep and uniform, and contained no suspended matter. Such water would be as black as ink. A reflected glimmer of ordinary light would reach us from its surface, as it would from the surface of actual ink; but no light, hence no colour, would reach us from the body of the water. In very clear and very deep sea water this condition is approximately fulfilled, and hence the extraordinary darkness of such water. The indigo, to which I have already referred, is, I believe, to be ascribed in part to the suspended matter, which is never absent, even in the purest natural water, and in part to the slight reflexion of the light from the limiting surfaces of strata of different densities. A modicum of light is thus thrown back to the eye, before the depth necessary to absolute extinction has been attained. An effect precisely similar occurs under the moraines of the Swiss glaciers. The ice here is exceptionally compact, and owing to the absence of the internal scattering common in bubbled ice, the light plunges into the mass, is extinguished, and the perfectly clear ice presents an appearance of pitchy blackness.

The green colour of the sea when it contains matter in a state of mechanical suspension has now to be accounted for; and here, again, let us fall back upon the sure basis of experiment. This white plate was once a complete dinner-plate, very thick and strong. It is, you see, surrounded securely by cord, and to it a lead weight is fastened. Forty or fifty yards of strong hempen line were attached to the plate.

With it in his hand, my assistant, Thorogood, occupied a boat fastened as usual to the davits of the 'Urgent,' while I occupied a second boat nearer to the stern of the ship. He cast the plate as a mariner heaves the lead, and by the time it had reached me, it had sunk a considerable depth in the water. In all cases the hue of this plate was green, not, of course, a pure green, but a mixture of green and blue; and when the sea was of the darkest indigo, the green was the most vivid and pronounced. I could notice the gradual deepening of the colour as the plate sank, but at its greatest depth in indigo water the colour was still a blue green.

Other observations confirmed this one. The 'Urgent' is a screw steamer, and right over the blades of the screw there was an orifice called the screw-well, through which you could look from the poop down upon the screw. The surface glimmer which so pesters the eye was here in a great measure removed. Midway down a plank crossed the screw-well from side to side, and on this I used to place myself to observe the action of the screw underneath. The eye was rendered sensitive by the moderation of the light; and still further to remove all disturbing causes, Lieutenant Walton had the great kindness to have a sail and tarpaulin thrown over the mouth of the well. Underneath this I perched myself, and watched the screw. In an indigo sea the play of colours was indescribably beautiful, and the contrast between the water which had the screw-blades for a background, and that which had the bottom of the ocean as a background, was extraordinary. The one was of the most brilliant green, the other of the most lustrous ultramarine. The surface of the water above the screw-blade was always ruffled. Liquid lenses were thus formed, by which the coloured light was withdrawn from some places and concentrated upon others. The screw-blades in this case replaced the plate in the former case, and there were other instances of a similar kind. The hue from an indigo sea was always green at a certain depth below the surface. The white bellies of the porpoises showed the same hue, varying in intensity as the creatures swung to and fro between the surface and the deeper water. In a rough sea the light which had penetrated the summit of a wave sometimes reached the eye. A beautiful green cap was thus placed upon the wave when the ship was in indigo water.

But how is this colour to be connected with the suspended particles? Take the dinner-plate which showed so brilliant a green when thrown into indigo water. Suppose it to diminish in size until it reached an almost microscopic magnitude. It would still behave substantially as the larger plate, sending to the eye its modicum of green light. If the plate, instead of being a large coherent mass, were ground to a powder sufficiently fine, and in this condition diffused through the clear sea water, it would send green to the eye. In fact, the suspended particles which the home examination revealed in green sea water act in all essential particulars like the plate, or like the screw-blades, or like the foam, or like the bellies of the porpoises.

When too gross, or in too great quantity, the suspended particles thicken the sea itself visibly. But when sufficiently small, but not too small, and when sufficiently diffused, they do not sensibly interfere with the limpid greenness of the sea itself. They then require the stronger and more delicate test of the concentrated luminous beam to reveal their presence.

Suspended Particles in Drinking-water.

The method employed here, and which is one of extreme simplicity, may be extended. It may of course be applied to the examination of ordinary water, and when this is done unexpected results are sometimes obtained. Here, for instance, is a bottle of water intended to quench the lecturer's thirst, and it would be well for the lecturer not to scrutinize it too closely. In the track of the beam of light it simply reveals itself as dirty water. So you see that we are invaded with dirt not only in the air we breathe, but in the water we drink. And this water is no worse than the other London waters. Thanks to the kindness of Professor Frankland, I have been furnished with specimens of the water of eight London companies. They are all laden with impurities mechanically suspended. But you will ask whether filtering will not remove the suspended matter? The grosser matter, undoubtedly, but not the more finely-divided matter. Here is water which has been passed four times through a filter of bibulous paper, but it is still laden with fine matter. Here, also, is a bottle kindly lent me by Mr. Lipscomb, and passed once through his charcoal filter. But the track of the beam through it is more luminous than through air, because the quantity of matter suspended in the water is greater than what is suspended in air. Here is another specimen courteously sent to me by the Silicated Carbon Company. All the grosser matter has been removed, but it is thick with fine matter. Nine-tenths of the light scattered by these particles is perfectly polarized in a direction at right angles to the beam, and this release of the particles from the ordinary law of polarization is a demonstration of their smallness. I should say by far the greater number of the particles concerned in this scattering are wholly beyond the range of the microscope. They are so small that I do not think that any filter can intercept them. I do not want to frighten you in any way, for our drinking-water may be harmless—it may be only nasty. There is an æsthetic pleasure in the drinking of a glass of cold sparkling water, and I fear these experiments will destroy this pleasure if you ever enjoyed it. And it is next to impossible by artificial means to produce a pure water. Mr. Hartley, for example, some time ago distilled water while it was surrounded by hydrogen, but the water was not free from floating matter. Here is a specimen obtained from the combustion of water in air, the aqueous vapour being condensed by the polished surface of a chilled silver basin. When shaken up it is full of fluff and particles, coarse and fine. It is so hard to be clean in the midst of dirt. Here, however, is an approach to pure water. It is from the Lake of Geneva,

and the bottle was carefully filled for me by my distinguished friend Soret. The water has remained quiet for a good while, and its purity is in part to be ascribed to the subsidence of the particles. The track of the beam through it is of a delicate sky-blue; there is scarcely a trace of grosser matter.

The purest water that I have obtained—probably the purest which has been seen hitherto—has been obtained from the fusion of selected specimens of ice. But extraordinary precautions are required to obtain this degree of purity. Here is an apparatus devised and constructed by Mr. Cottrell for this purpose. Through the plate of an air pump passes the shank of a large funnel, attached to which below the plate is a series of glass bulbs. In the funnel is placed a block of the most transparent ice, and over the funnel is a glass receiver. This is first exhausted and refilled several times with air, which has been filtered by its passage through cotton wool, the ice being thus surrounded by absolutely pure moteless air. But the ice has previously been in contact with mote-filled air; it is therefore necessary to let it wash its own face, and wash the bulb which is to receive the water of liquefaction. The ice is permitted to melt, the bulb is filled several times and emptied several times, until finally the large block dwindles to a small one. We may be sure that all impurity has been thus removed from the surface of the ice. These two bulbs contain water obtained in this way, the purity of which is the maximum hitherto attained. Still I should hesitate to call the water absolutely pure. When the concentrated beam is sent through it the track of the beam is not invisible, but of the most exquisitely delicate blue. This blue is purer than that of the sky, so the matter which produces it must be finer than that of the sky. Looked at perpendicularly to the beam the blue may be quite quenched by a Nicol's prism. It may be, and indeed has been, contended that this blue is scattered by the very molecules of the water, and not by matter suspended in it. But when we remember that this perfection of blue is approached gradually through stages of less perfect blue, and when we remember that a blue in all respects similar is demonstrably obtainable from particles mechanically suspended, we should hesitate, I think, to conclude that we have arrived here at the last stage of purification. The evidence, I think, points distinctly to the conclusion that could we push the process of purification still farther, even this last delicate trace of blue would disappear.

Dr. Bence Jones proposed to me a few days ago to express numerically the degrees of impurity arising from mechanical suspension. This ice water might be called the standard, or "1," all higher degrees of impurity being marked by higher numbers. Such a method would undoubtedly give distinctness to our language in speaking of water. But my aim is to point out a means of extreme simplicity by which microscopic examination may be powerfully aided and supplemented. Five minutes' inspection of a water in this way will teach you more regarding the quantity of impurity than whole days spent with the

microscope. The microscope is rightly employed in determining the character of the individual particles which are revealed *en masse* by the luminous beam.

Drinking-water from Chalk Districts.

But is it not possible to match the water of the Lake of Geneva here in England? Undoubtedly it is. We have in England a kind of rock which constitutes at once an exceedingly clean recipient and a natural filter of extraordinary efficacy, and from which we can obtain water extremely free from mechanical impurities. I refer to the chalk formation, in which large quantities of water are held in store. Our chalk hills are in most cases covered with thin layers of soil, and with very scanty vegetation. Neither opposes much obstacle to the entry of the rain into the chalk, where any organic impurity which the water may carry in is soon oxidized and rendered harmless. Those who have scampered like myself over the downs of Hants and Wilts will remember the scarcity of water in those regions. In fact, the rainfall, instead of washing the surface and collecting in streams, sinks into the fissured chalk and percolates through it, and when this formation is suitably tapped we obtain water of exceeding briskness and purity. Here is a large globe filled with the water of a well near Tring. It is wonderfully free from mechanical impurity; indeed, it stands to reason that water wholly withdrawn from surface contamination and percolating through so clean a substance should be pure. Sending a beam through this glass of water its purity is conspicuous; you see the track of the beam, but it is not the thick and muddy track revealed in London waters. It has been a subject much debated whether the supply of excellent water which the chalk holds in store could not be rendered available for London. Many of the most eminent engineers and chemists have ardently recommended this source, and have sought to show that not only is its purity unrivalled but that its quantity is practically inexhaustible. Data sufficient to test this are now, I believe, in existence; the number of wells sunk in the chalk is so considerable and the quantity of water which they yield is so well known.

But this water, so admirable as regards freedom from mechanical impurity, labours under the disadvantage of being very hard. It is rendered hard by the large quantity of carbonate of lime which it holds in solution. The chalk water in the neighbourhood of Watford holds in solution about 17 grains of carbonate of lime per gallon. This, in the old terminology, used to be called 17 degrees of hardness. Now this hard water is bad for tea, bad for washing. It furs your boilers, because the lime held in solution is precipitated by boiling. If the water be used cold its hardness must be neutralized at the expense of soap before it will give a lather. I remember being struck by the enormous quantity of soap wasted in a washing establishment in Hampshire through the hardness of the water. This of itself would seem an insuperable objection to the use of chalk water in

London. But the objection has now been successfully met by the experimental demonstration that such water can be softened inexpensively and on a grand scale. I had long known the method of softening water called Clark's process, but not until recently did I see proof of its application in the case of large communities. The chalk water is softened by Mr. Homersham at Caterham, at Canterbury, and at the Chiltern Hills for the supply of Tring, Aylesbury, and other places.

I have visited all these places, and made myself acquainted with the works. At Canterbury there are three reservoirs covered in and protected by a concrete roof and layers of pebbles both from the summer's heat and the winter's cold. Each reservoir contains 120,000 gallons of chalk water. Adjacent to these reservoirs are others containing pure slaked lime—the so-called "cream of lime." These are filled with water, the lime and water being thoroughly mixed by air forced in by an engine through apertures in the bottom of the reservoir. The water thus well mixed with the lime soon dissolves all of this substance that it is capable of dissolving. The lime is then allowed to subside to the bottom, leaving a perfectly clear lime water behind.

The object is now to soften the chalk water. Let us suppose one of the three large reservoirs empty; into this is introduced a certain quantity of the clear lime water, and after this about nine times the quantity of the chalk water. The transparency immediately disappears—the mixture of the two clear liquids becomes thickly turbid. The carbonate of lime is precipitated, and the reservoir being now full the precipitate is permitted to subside; it is crystalline and heavy, and therefore sinks rapidly. In about twelve hours you find a layer of pure white carbonate of lime at the bottom of the reservoir, with a water of extraordinary beauty and purity overhead. A few days ago I pitched some halfpence into a reservoir 16 feet deep at the Chiltern Hills. The 16 feet hardly perceptibly dimmed the coin. Had I cast a pin in, it could, I am persuaded, have been seen at the bottom. By this process of softening the water is reduced from about 17 degrees of hardness to 3 degrees of hardness. It yields a lather immediately. Its temperature is constant throughout the year. In the hottest summer it is cool; it does not freeze in winter if conveyed in proper pipes. It is not exposed to the contamination of either earth or air. The reservoirs are covered; a leaf cannot blow into it, no surface contamination can reach it, it passes direct from the main into the house-tap; no cisterns are employed, the supply is always fresh and pure. It is highly charged with air. This is the kind of water which is supplied to the fortunate people of Canterbury.

Let me show you a specimen of this softened water, and as the eye is eminently an organ of relation, needing comparison to judge aright, I will subject to the gauge-test a globe of water taken out of our cistern down-stairs, and a globe of water taken a few days ago from one of the reservoirs at the Chiltern Hills. Here are two electric

lamps, connected with two batteries down-stairs. I send the beams through both of them, first permitting the globes themselves to act as lenses, and afterwards converging the beams upon them with glass lenses. You can judge of the difference without any word of mine.

The Water Supply of London.

The purity and goodness of this chalk water are demonstrable, and if we like it hard we can have it hard; if we like it soft we can have it soft. It lends itself to our purposes with perfect flexibility; the sole doubtful question is the adequacy of the supply for three millions of people. But let us take some common ground. On all hands it will be admitted that large—I will not say "vast" quantities of this beautiful water run to waste. Why should this be permitted? If the chalk is unable to supply the wants of three millions, it is certainly able to supply the wants of a smaller number. Supposing it competent to promote health, cleanliness, and comfort among one-third of the population of London, why not apply it to these uses? Why should a valuable treasure be lost, because it does not possess three times its actual value?

Let your expectations be as moderate as you like, only make the trial. That trial, of which the success is demonstrable, will solve the larger question whether the chalk is able to supply the whole of London. In moral matters it is our duty to realize our best insight. Let us do so here. By honestly doing what we can with this chalk water, we shall learn the limits of our action. A small good is certainly within our reach, and by accepting it we may disclose possibly an incalculable benefit to London.

[J. T.]

WEEKLY EVENING MEETING,

Friday, January 27, 1871.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

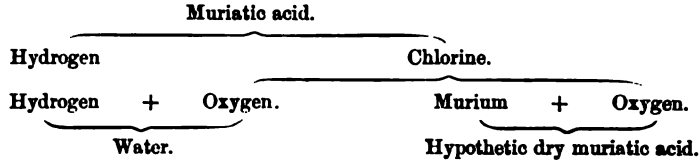
WILLIAM ODLING, M.B. F.R.S.

FULLERIAN PROFESSOR OF CHEMISTRY, ROYAL INSTITUTION,

On Improvements in the Manufacture of Chlorine.

CHLORINE gas was first obtained in 1774 by the Swedish chemist, Scheele, as a product of the action of muriatic acid—or marine acid, as it was then called—on the black earthy mineral manganese. In accordance with the theoretical notions of the day, Scheele gave to his newly-discovered aerial substance the name dephlogisticated marine acid, which, translated into the language of the new Lavoisierian

school, became soon afterwards oxy-muriatic acid. Its assumed constitution, and relationship to muriatic acid are indicated below.



But Davy, in 1810, showed that muriatic acid gas is a substance wholly free from water, and a definite compound of Scheele's gas with hydrogen. He showed further that Scheele's gas, the so-called oxy-muriatic acid, was not only incapable of furnishing oxygen under any circumstance whatever; but that, so far as experience went, it was altogether undecomposable. Being thus an undecompounded and apparently undecomposable substance, he proposed to regard it as an element, and to designate it, from its yellowish-green colour, by the name chlorine, which it has ever since borne. After considerable opposition, especially on the part of Berzelius, who subsequently, however, became a convert, the view of Davy at length prevailed, and has ever since held its ground, not indeed without occasional challenging from time to time. Thus Schönbein, in several of his letters to Faraday, maintained very strongly the superior truthfulness of the original view; and still more recently Brodie has represented the comparable units of hydrogen H_2, muriatic acid HCl, and chlorine Cl_2, by the symbols α, $\alpha\chi$, and $\alpha\chi^2$, implying that the units of muriatic acid and chlorine differ from the unit of hydrogen by a combination of the matter of α with the matter of χ and with the matter of χ^2, respectively. But whatever may hereafter prove to be its real nature, chlorine has hitherto proved undecomposable, and is accordingly regarded by the generality of chemists as an element.

Coincident with the discovery of chlorine itself was the discovery of its most characteristic property, that of bleaching vegetable colours. The petals and leaves of plants, infusions of their several colouring matters, as well as fabrics dyed with the several colouring matters, were found to be rapidly bleached when treated either with chlorine gas, or with a solution of the gas in water. From the rapid action of the gas and its solution on pronounced vegetable colours, Berthollet in 1785 conceived the notion that chlorine in one form or other might be used with advantage commercially for the bleaching of calicoes and linens, theretofore always bleached by a tedious exposure to air and sunlight. The processes devised by Berthollet for carrying out the new method of bleaching were introduced into Scotland in 1787, and soon afterwards became extensively employed. Woven fabrics were bleached by immersion in chlorine water, and paper-pulp more especially by exposure to chlorine gas.

The bleaching action of chlorine takes place only in presence

of water or moisture, and is dependent on the property of chlorine under suitable circumstances to decompose water, by uniting with its hydrogen to form muriatic acid, and so setting free its oxygen, by which the bleaching is really effected. Thus when chlorine, blue indigo, and water are present together, the opposite tendencies of the chlorine to take hydrogen, and of the indigo to take oxygen from the water, are satisfied by the production of chloride of hydrogen or muriatic acid on the one hand, and of oxide of indigo or isatin on the other, this isatin being a soluble orange-red non-tinctorial body:

$$Cl_2 + H_2O + C_8H_5NO = 2HCl + C_8H_5NO_2.$$

Thus bleaching by the action of chlorine, like bleaching by exposure to air, is really an act of oxidation.

But the action of chlorine on vegetable fabrics is not confined to that of bleaching their attached colouring matter; the fibre of the fabric is itself liable to be attacked both by the free chlorine and the muriatic acid to which it gives origin. Hence it became important to find a substitute for the free chlorine which should have, like it, the property of liberating nascent oxygen so as to bleach, but should not have, at any rate in the same degree, the property of corrosiveness so as to destroy. Such a substitute is offered in the familiar compound known as bleaching powder, introduced into commerce by the Messrs. Tennant, of Glasgow, in the year 1799. This substance is made by exposing slaked lime to an atmosphere of chlorine gas. The chlorine enters into combination with the lime of the slaked lime, to furnish a product which in practice contains some 35 per cent. of its weight of chlorine, and consists of a definite compound of lime with its own weight of chlorine, plus a variable excess of slaked lime unacted upon:

$$CaO \cdot H_2O + Cl_2 = CaOCl_2 + H_2O.$$

Bleaching powder occurs as a dry white pulverulent solid, having a characteristic smell, allied to but not identical with that of weak chlorine. It dissolves considerably in water to form a yellow-tinted liquid, known as bleaching liquor, having a marked alkaline reaction from the presence in it of excess of lime. Coloured goods immersed for a longer or shorter time in bleaching liquor of greater or less strength and temperature, gradually have their colouring constituents destroyed or oxidated, the bleaching salt $CaOCl_2$, losing its oxygen, and becoming changed into chloride of calcium, $CaCl_2$. It is observable that the bleaching or liberation of nascent oxygen from bleaching powder is much slower than the bleaching or liberation of nascent oxygen by means of free chlorine; to such an extent, indeed, that for many purposes bleaching powder would be quite inapplicable, were it not for the property it has of liberating chlorine on acidification, whereby a still indirect but yet almost immediate oxidation is effected. Ten grains of bleaching powder, for instance, mixed with a

little sulphuric or muriatic acid, will evolve some four cubic inches of chlorine gas:

$$CaOCl_2 + H_2SO_4 = H_2O + Cl_2 + Ca''SO_4.$$

The superior activity of the chlorine liberated by acidification of bleaching liquor to the original bleaching liquor, is taken advantage of commercially. Brown linens, for example, are first steeped in weak bleaching liquor with little apparent result, and then "soured" by immersion in dilute acid, whereby an immediate more or less complete bleaching is effected. Again, in what is known as the "discharge style" patterns or designs are printed with a mixture of gum and tartaric or other acid, upon a red or blue ground, and the printed fabric passed through a solution of bleach. The red or blue ground withstands the feebler action of the bleach, but the acid printed pattern becomes completely decolorized by the chlorine set free locally, as a result of the action of the acid upon the bleach. It is to this facility of management and capability of being used in different ways that the great utility of bleaching powder and its consequent enormous consumption are due. The amount of bleaching powder made annually in Great Britain and Ireland at the present time is estimated at 75,000 tons, representing at the price of ten pounds a ton, a value of three-quarters of a million sterling.

Bleaching powder being formed by exposing slaked lime to an atmosphere of chlorine, the manufacture of bleaching powder involves the production of chlorine. Indeed, chlorine is produced at chemical works almost exclusively for the manufacture of bleaching powder, and of one other product, namely, chlorate of potash. This latter is made to the extent of about 750 tons per annum, having at the price of one hundred and five pounds per ton, a value of nearly 80,000*l.* sterling.

The original reaction upon one another of muriatic acid and peroxide of manganese, by which Scheele was led to the discovery of chlorine, is the basis of the manufacturing process which has ever since been almost exclusively followed. The action consists essentially in the excessive oxygen of the peroxide taking away the hydrogen of the muriatic acid and so setting its chlorine free. Peroxide of manganese, more often called simply manganese, is one of that very limited class of native mineral substances containing an excess of oxygen; the only other similarly peroxidized substance abundant in nature being nitre, in its two forms of East Indian or potash-nitre, and Chilian or soda-nitre. Now, when muriatic acid instead of being acted on by manganese is acted on by nitre, or by the nitric acid, formerly known as spirit of nitre, produced therefrom, the excessive oxygen of the nitre takes away hydrogen from the muriatic acid and sets its chlorine free. Thus by addition of nitric acid or nitre to aqueous muriatic acid a solution of chlorine is produced, recognizable in various ways, and especially by its power of dissolving gold leaf. Now, although nitre or nitric acid is a much more expensive oxygenant

than manganese it was conceived that nitric acid might nevertheless be substituted with advantage for manganese, from the circumstance that after it had given up its excessive oxygen so as to liberate the chlorine of the muriatic acid, its deoxidized residue could readily acquire fresh oxygen from the atmosphere, and so reproduce nitric acid, to be used as an oxygenant over and over again indefinitely; and, indeed, the nitric method of producing chlorine has for some time past been to a limited extent in actual use.

With regard to manganese, however, until recently no means have been known of causing its deoxidized residue to re-acquire oxygen from the air, and so become in a condition to serve again for the liberation of chlorine. From time to time, more particularly by Messrs. Tennant and Mr. Wm. Gossage, attempts have been made in this direction, some of them so far successful as to achieve a limited adoption; and at length a process of reviving the spent manganese residue by effecting its aerial oxidation, has proved a decided commercial success. This process, introduced by Mr. Walter Weldon, has the incidental advantage of doing away with the discharge of the extremely corrosive manganese liquor into streams and watercourses, whereby great damage was frequently occasioned. It consists substantially in neutralizing with chalk, the acid liquor left by the action of the muriatic acid on the manganese, and then pumping air through the neutralized liquor mixed with excess of lime. The original acid liquor is substantially a solution of chloride of manganese with perchloride of iron, chloride of aluminum, chloride of calcium, and the excess of chloride of hydrogen, or muriatic acid, employed. By neutralization of the acid liquor with chalk, the iron of the perchloride of iron is precipitated, and a pale pink manganous solution left. By treatment of this solution with milk of lime, an almost white precipitate, consisting of hydrated protoxide of manganese with excess of hydrate of lime, is thrown down. Then by blowing air through the resulting thin sludge, raised to the temperature of 140°–160° F. by injection of steam, oxygen is rapidly absorbed, and the sludge rendered of a deep black colour. By subsidence the now black sludge yields a dense deposit, from which the supernatant clear liquid is run off. Finally, the deposit in its moist condition, and containing in every cubic foot about four pounds weight of peroxide of manganese, is acted upon by muriatic acid in the chlorine stills, and the resulting acid liquid treated over again in the same way, and so on indefinitely.

Already the Weldon process of manganese restoration is employed and about to be employed at many large works, to such an extent that before very long one-half of the entire quantity of bleaching powder produced in the kingdom may be expected to be made with chlorine generated by means of the restored manganese. It is noteworthy that the original pale precipitate of hydrated protoxide of manganese undergoes oxidation very imperfectly save in presence of a considerable excess of lime, and that the black product finally obtained is not simply peroxide of manganese MnO_2, but a compound of the peroxide

with lime, of composition varying in different instances between that indicated by the respective formulæ $CaO.MnO_2$ and $CaO.2MnO_2$, a small portion of the lime CaO being usually replaced by its equivalent of protoxide of manganese MnO. Some idea of the rapidity of oxidation effected under favourable circumstances may be gathered from the following result. Of the entire quantity of oxygen contained in 175,000 cubic feet of air blown into the sludge within five hours, 14·8 per cent., equal to rather more than four hundredweights, was absorbed in the production of twenty-two hundredweights of manganese peroxide MnO_2. It is obvious that where Weldon's process is carried out, what happens is this:—The original peroxide of manganese first oxidizes a quantity of muriatic acid, and sets free its chlorine. Then the spent manganese is caused to acquire a fresh dose of oxygen from the air, and is used to oxidize a fresh quantity of muriatic acid, and so on continuously. Thus, except during the first use of the manganese, the muriatic acid is really oxidized by the oxygen of the air, the manganese serving only as a carrier. Latterly, Mr. H. Deacon, of the Widnes Alkali Works, has conceived the idea of dispensing with the carrier—that is the manganese—altogether. It has been known for some time that when muriatic acid vapour mixed with air is passed through heated tubes, the oxygen of the air effects a partial oxidation of the muriatic acid, setting free its chlorine; but the reaction is not sufficiently complete to render the process commercially available. Mr. Deacon however has ascertained that when a mixture of air and muriatic acid vapour, heated to about 700° F., is passed over a mass of brickwork that has been steeped in solution of sulphate of copper and afterwards dried, an almost complete decomposition of the muriatic acid is effected. The copper salt certainly takes part in the reaction as an intermediary, but in the end is quite unaffected, the sole obvious result being that the chlorine of the muriatic acid is set free by the oxygen of the air, with a rapidity which seems to leave nothing to be desired. The nature of the intermediary action exerted by the copper-salt, so ingeniously made serviceable by Mr. Deacon, has not been clearly ascertained. It would appear, however, that by two simultaneous reactions taking place continuously throughout the process, chloride of copper is ever being alternately formed and decomposed; it being remembered that chloride of copper gives off a portion of its chlorine with much facility, even by heat alone. The extreme result would probably be somewhat as follows:

$$\begin{aligned} CuCl_2 + O &= CuO + Cl_2 \\ CuO + 2HCl &= CuCl_2 + H_2O \end{aligned}$$

Although this new process has not yet been worked on a manufacturing scale, considerable experience has been acquired with regard to it, and the difficulties to which it at first seemed liable have been successfully provided against. One inevitable difficulty arising

from the diluted state of the produced chlorine, has proved to be less serious than was anticipated. This state of dilution is at all times considerable; for the chlorine being generated by the action of atmospheric oxygen on muriatic acid, every volume of chlorine produced even in an absolutely perfect working of the process, must be necessarily diluted with twice its volume of atmospheric nitrogen. Nevertheless, according to Mr. Deacon's experience, the diluted state in which the chlorine is always obtained, has not been found to interfere with its application to the manufacture either of chlorate of potash or bleaching powder. The requisite plant for carrying out the invention on a large scale is now being erected at Messrs. Gaskell and Deacon's works.

For experimental purposes the new process of continuous chlorine production may be performed with oxygen instead of air. It then manifests a striking reverse relationship to a process of continuous oxygen production, in which oxide of cobalt acts as a permanent intermediary. In the one experiment the gas goes in oxygen and comes out chlorine, muriatic acid being decomposed and water formed. In the other experiment the gas goes in chlorine and comes out oxygen, water being decomposed and muriatic acid formed. In the presence of slaked lime to arrest the muriatic acid, this last reaction takes place readily and completely below the boiling point of water.

[W. O.]

WEEKLY EVENING MEETING,

Friday, February 3, 1871.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

W. Spottiswoode, Esq. M.A. Treas. R.S. and R.I.

On some Experiments on Successive Polarization of Light made by Sir C. Wheatstone.

The experiments which formed the subject of this discourse were made by Sir Charles Wheatstone some years ago, but the pressure of other avocations delayed their publication. The term "Successive polarization" was applied by Biot to denote the effects produced when a ray of polarized light is transmitted through a plate of rock crystal cut perpendicularly to the axis. The plane of polarization is found to be changed on emergence and through a different angle for each homogeneous ray. The introduction of instrumental means for converting the plane polarization of the ordinary apparatus into successive, or, as it is more commonly called, circular polarization, and the explanation of the phenomena thence arising, constitute the main purpose of the communication.

Polarized light is distinguished from common light by the presence of certain peculiarities not ordinarily found; but the peculiarities in question cannot be discerned by the unassisted eye, and require special instrumental appliances for detection. A simple mode of bringing light into the condition in question is by allowing it to pass through a plate of the crystal called tourmalin, cut parallel to the axis; and if the light be then examined by causing it to pass through a second similar plate, held parallel to the former and caused to revolve like a wheel in its own plane, it will be found that the intensity alternately diminishes and revives, being zero for two positions 180° from one another, and a maximum for two positions at 90° from each of the former. Thus combination of tourmalins constitute in fact a polariscope, which in general consists of two parts, counterparts of each other, the first for bringing common light into the condition in question, in other words for *polarizing*; the second for examining or *analyzing* the light.

The explanation of this fundamental phenomenon is as follows:—The vibrations upon which the sensation of light depends, may in ordinary light take place in any direction in a plane perpendicular to the ray. By the process of polarization they are all brought into one direction, still, however, perpendicular to the ray; so that throughout the entire ray they lie in one plane. On this account the polarization here considered is called plane polarization. There are other kinds of polarization, such as circular and elliptic, whose names are derived from the curves, or orbits, described by the vibrating particles.

There are also other methods for producing plane polarization beside that above described; *e. g.* reflexion at particular angles from the surfaces of transparent media, transmission through parallel plates of glass, &c.; but as they all agree in reducing common light to the same condition, it is unnecessary for the present purpose to allude to them more in detail.

If a ray of polarized light fall upon a plate of doubly-refracting crystal, it is divided into two, whose vibrations lie in planes perpendicular to one another. These rays traverse the crystal with different velocities, and therefore emerge with a difference of phase. On entering the analyzer the vibrations of both rays are resolved into one plane. If the plane of vibration of the analyzer be parallel to one of those of the crystal, one ray will be cut off, the other will be transmitted without change. In any other position of the analyzer the transmitted portions of the two rays will interfere so as to produce colour; and if the analyzer be then turned through 90°, the portion of the original light cut off in the first position will be transmitted, and *vice versâ*.

Of this theory the following are the experimental results:—If a plate of doubly-refracting crystal, *e. g.* selenite, be placed between the polarizer and analyzer, and turned round in its own plane, it will be found that in certain positions at right angles to one another no effect is produced. These may be called neutral positions. In all other posi-

tions the field is tinted with colour, which is most brilliant when the plate has been turned through 45° from a neutral position. If the analyzer be turned, the crystal remaining still, the colour will fade, and entirely vanish when the angle of turning amounts to 45°. From this position the complementary colour will begin to appear, and will be brightest when the angle of turning amounts to 90°. The colour depends upon the thickness of the crystal, so that by a suitable preparation any arrangement of colours may be produced.

So far for plane polarization. The principle of circular or successive polarization, as regards the present purpose, is as follows:—

If two sets of rectilinear vibrations lying in planes perpendicular to one another meet and combine, the resulting vibration will be curvilinear, whose form and position depends upon the difference of phase of the components. If the second set be in advance or in rear of the first by a quarter of a wave length, the resulting vibration will be circular; but the motion will in one case be direct (like the hands of a watch), in the other it will be reverse.

If two sets of circular vibrations in opposite directions meet and combine, the resulting vibrations will be rectilinear, and the position of its plane will depend upon the difference of phase of the components. If the second set advance upon the first, the plane of resultant vibration will undergo direct rotation; if it recede, it will undergo reverse rotation.

If in such an experiment white light be used, the vibrations of the different component prismatic colours will (on account of their different wave lengths) undergo different retardation; and consequently the resulting vibrations will lie in different planes, arranged in prismatic order. The order will be from red to violet, or *vice versâ*, in accordance with the law stated above.

If a ray of plane polarized light fall upon a metallic reflector, it is divided into two, whose vibrations are respectively parallel and perpendicular to the reflector; and the latter is retarded behind the former by a difference of phase depending upon the angle of incidence. If the plane of vibration of the incident ray be inclined at an angle of 45° to the plane of incidence, the two rays into which it is divided have nearly the same intensity. At an angle, nearly 45°, which varies with the metal employed, but which is perfectly definite, the intensities become accurately equal. And further, if the angle of incidence have a particular value dependent upon the nature of the metal (for silver 72°), the retardation will amount to a quarter of a wave length. These two rays, on leaving the reflector, will recombine, and in accordance with the laws above given, will in the last-mentioned circumstances become a circularly polarized ray. Lastly, the direction of motion in this circular ray will depend upon the side on which the original plane of vibration is inclined to the plane of incidence; if, when it is inclined on one side, the circular ray becomes right-handed, then, when it is inclined on the other, it becomes left-handed.

Reverting then to the phenomena of double refraction produced

by a plate of crystal cut parallel to the axis on a plane polarized ray; let the crystal be placed in such a position that the planes of vibration of the two resulting rays are inclined at angles of 45° on the two sides respectively of the plane of incidence; and let there be interposed between the crystal and the analyzer a silver plate at an angle of 72° to the direction of these emergent rays. Each of these rays will, in virtue of the principles enunciated above, be converted by reflexion into a circularly polarized ray; but one will be a right-handed and the other a left-handed ray; and the difference of phase produced by the doubly-refracting plate will be undisturbed by the reflexion. This difference of phase depends, as is well known, upon the wave length, in other words, upon the colour of the light. So that the two circular rays will combine to form a plane polarized ray, whose plane of vibration depends upon the difference of phase, *i. e.* upon its colour. And if, finally, the light be then examined by an analyzer in the usual manner, we shall have all the phenomena of circular or successive polarization.

From what has been stated above, it appears that the direction of motion in the two circular rays, and consequently the order of colours produced, depends upon the position (to the right or left of the plane of incidence) of the ray which has been most retarded in the passage through the crystal plate. If, therefore, the plate being in a given position, the colours appear in an ascending order, then on turning the plate through 90° in its own plane, or on turning it over about an axis in the plane of incidence, the swifter and the slower rays will change position, and the order of colours will be reversed.

The reversal of the order of colour may be exhibited in another way. Uniaxal crystals are divided into two classes: one, called positive (*e. g.* quartz), in which the extraordinary ray moves more slowly than the ordinary; the other, called negative (*e. g.* Iceland spar), in which the ordinary ray is the slowest. If, therefore, a plate of quartz placed with its axis at 45° on one side of the plane of incidence give the colours in one order, a similar plate of Iceland spar similarly placed will give them in the reverse order.

The same principles apply to the case of biaxal crystals cut parallel to a plane containing the two optic axes. A ray of plane polarized light transmitted through such a plate is divided into two, whose vibrations respectively bisect the angles formed by the two axes; the line which bisects the smallest angle is called the intermediate section, and the line perpendicular to it the supplementary section; and the order of the colours depends upon the relative velocity of the two rays. In selenite the ray whose vibrations lie in the supplementary section is the slowest; in mica it is the swiftest. Hence these two crystals will, all other circumstances being alike, give opposite orders of colour, and may be regarded as positive and negative respectively, like quartz and Iceland spar.

The phenomena by which these principles may be illustrated are very numerous and varied, but are better seen than described.

[W. S.]

GENERAL MONTHLY MEETING,

Monday, Feb. 6, 1871.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Henry Edward Colvile, Esq.
John H. Dallmeyer, Esq.
Duncan Davidson, Esq.
Warren William De la Rue, Esq.
James N. Douglass, Esq.
Mrs. Gibbs.
Abraham Goodall, Esq. F.R.C.S.
Alexander Macfarlan, Esq.
Robert Turtle Pigott, Esq., F.R.G.S.
Robert Sabine, Esq.
Charles Southwell, Esq.
Henry Stilwell, Esq.
Richard Valpy, Esq.
Mrs. Jacob Waley.

were *elected* Members of the Royal Institution.

The special thanks of the Members were returned for the following addition to "the Donation Fund for the Promotion of Experimental Researches":—

W. D. Esq. (*5th Donation*) £5 5 0

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:

FROM

Academia Pontificia de' Nuovi Lincei, Roma—Anno XXIII. 4to. 1870.
Admiralty, Lords of the—Nautical Almanac, 1874. 8vo. 1870.
Allen, Peter, M.D. M.R.I. (the Author)—Lectures on Aural Catarrh. 16mo. 1871.
Antiquaries, Society of—Proceedings, Vol. IV. No. 9. 8vo. 1870.
Asiatic Society of Bengal—Journal, Nos. 163, 164. 8vo. 1870.
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WEEKLY EVENING MEETING,

Friday, February 10, 1871.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

E. J. REED, Esq. C.B.

On some Fallacies connected with Ships and Guns.

IN opening his discourse Mr. Reed stated that the fallacies connected with ships, to which he proposed to draw attention, grouped themselves chiefly about that question of stability which the loss of the 'Captain' has recently brought into so much prominence, and at once proceeded to explain the nature of statical stability and the modes of measuring it.

In its simplest form the stability of a ship might be considered as the result of the total weight of the ship acting downwards through her centre of gravity, and of her total buoyancy (equal to the weight) acting upwards through a centre situated at a short horizontal distance from it, this distance defining the leverage with which either force acted about the other. The weight of the ship being the same whatever the inclination might be, the distance in question, viewed as a lever, or, in other words, the length of the lever, might be regarded as itself indicating the degree of "righting force" which the ship possessed. The actual lengths of these levers were given by Mr. Reed for the 'Captain' for all degrees of inclination at intervals of 7 degrees, and were as follows:—

At 7 degrees its length was 4¼ ins.		At 35 degrees its length was 7¼ ins.	
„ 14 „ „	8¼ „	„ 42 „ „	5¼ „
„ 21 „ „	10¼ „	„ 49 „ „	2 „
„ 28 „ „	10 „	„ 54½ „ „	nil.

It will here be seen that the length of the righting lever, which Mr. Reed spoke of as the "Arm of Safety" of the ship, increased up to 21 degrees of inclination, and then began to decrease, the cause of this being next explained, and shown to result solely from the lowness of the freeboard. All the time the ship had bulk to immerse on the depressed side the buoyancy went on increasing on that side, and the lever in question lengthened. When she had no more side to immerse, but was still inclined farther by the wind, the ship began to sink lower in the water, and re-immerse the opposite side, and consequently the buoyancy was gradually transferred back to that side, the lever meanwhile shortening, and the power of the ship to resist the wind

diminishing. The "Arm of Safety" consequently never reached a greater length than 10¼ inches, became only 5¼ inches at 42 degrees, and disappeared at 54½. This state of things was then compared with that of the 'Monarch.' The lengths of the corresponding levers in her were given as below:—

At 7 degrees its length was 4	ins.		At 35 degrees its length was 21⅝	ins.
„ 14	„	„ 8¼	„	„ 42	„	„ 22	„
„ 21	„	„ 12¼	„	„ 49	„	„ 20	„
„ 28	„	„ 18¼	„	„ 54½	„	„ 17½	„

In this ship, therefore, the "Arm of Safety," which is rather less than the 'Captain's' at small angles of inclination, instead of shortening like hers when only 10¼ inches, goes on lengthening to very nearly double that amount, attaining its maximum only at about 40 degrees, where the 'Captain's' leverage was rapidly disappearing; and there the 'Monarch' possessed double the greatest amount of stability which the 'Captain' ever possessed. Mr. Reed remarked that these figures fulfilled the double purpose of showing that the two ships were remarkably alike in stability all the time the low freeboard did not interfere; but as soon as that began to play its part, the resemblance between them ceased altogether. He next proceeded to show the unfitness of the low-sided ship to have her stability substantially corrected by ballast, although a high-sided ship could readily have a deficiency of stability made good by these means. In illustrating this, he compared the stability of the 'Captain,' with 400 tons of water-ballast admitted into her double bottom, with that of the 'Vanguard' class, with their iron ballast, as now equipped—about 360 tons in amount. The introduction of the 'Captain's' ballast so far improved her as to lengthen her "Arm of Safety" by a small amount at all degrees of inclination, but it left the general character of her curve of stability unaltered. In the case of the 'Vanguard,' on the other hand, her leverage of safety went on increasing up to great angles, becoming greater even than the 'Monarch's,' and presenting the very opposite condition to that of the 'Captain.' In all the aspects of statical stability the 'Captain' therefore stood alone, and bears no analogy or relation whatever to either the 'Monarch' or the broadside ironclads. Mr. Reed next explained by what means the stability of ships is increased, and stated that such an arrangement of weights might be made as to lower the centre of gravity and give to a ship of the 'Captain's' type a greater degree of safety than she possessed, but that it was not possible without exact calculations, based on specific designs, to say how low in freeboard it would be safe to go with a given spread of canvas. He incidentally remarked that as a matter of fact the 'Captain's' centre of gravity was much lower than that of several ironclads, giving the following figures:—

'Minotaur,'	distance of centre of gravity below load water line	..	1·99
'Valiant,'	„	„	„	.. 1·89
'Achilles,'	„	„	„	.. 1·51
'Captain,'	„	„	„	.. 3·25

It might, nevertheless, have been placed lower if the necessity for doing so had been foreseen. On the other hand, changes involving a raising up of great weights would have been essential in a new ship of the type, because on the eve of the loss, Captain Coles admitted to the Commander-in-Chief that the guns and turrets of the ship were much too low. Mr. Reed next explained the nature of dynamical stability, and how it was measured, and discussed the effects first of a rising wind, and then of gusts and squalls, upon ships under canvas. He showed that when inclined to 14 degrees the 'Monarch' had dynamical stability remaining to enable her to resist a squall of sufficient force, if continued, to hold her over to an angle of 20 degrees or more, and even then to have a large reserve of stability still in store. The 'Captain,' on the other hand, was without any such reserve, and would necessarily roll over under such a squall. Exact calculations only could show what reserve of force a ship had, and all these calculations affecting the 'Captain' have been made from drawings of the ship as built since her loss. The necessity for them was not foreseen in the proper quarter. This portion of the lecture concluded with a brief discussion of the effect of waves upon ships sailing under canvas, and of the extreme difference between such ships when with sails and without, showing how little doubt the 'Captain's' loss need cast on the unmasted monitors of the navy.

Another fallacy which also related to the question of stability, but which the speaker considered extremely unscientific, and almost puerile, is that which attributes certain danger to the existence in a ship of a double-bottom divided into watertight cells or compartments. Gentlemen, whose names and whose very professions he refrained from mentioning, had published papers in which the doctrine is laid down that a cellular bottom is radically and necessarily dangerous; and, unhappily, such writings have in these days an influence which every man of science must deplore. It is difficult to treat such opinions with any respect, but the speaker endeavoured, in a few sentences, to illustrate their irrationality. He thought the best way to proceed was to assume that the logical way of remedying any evil which may attend the presence of two bottoms will be to remove one of them. He first supposed the inner bottom of the 'Captain' to have been removed, and inquired what effect this would have upon the ship. To his mind the effect was obvious. This inner bottom being substantially constructed of iron, is of very considerable weight, and the removal of this weight from the very bottom of the ship would have had the certain effect of raising the centre of gravity, diminishing the stability, and capsizing the ship all the earlier. There cannot be a doubt on the question. If it should be said that by removing this bottom the means would have been afforded of placing the engines, boilers, and other weights lower down in the ship than they could be placed while the inner bottom existed, he would answer that this line of argument is based upon a total misapprehension. It assumes that the weights of the 'Captain' and other ironclads were

unavoidably placed objectionably high, because of the existence of the double bottom. The very contrary is the case. The distance between the double bottoms has been made great in recent ironclads expressly to facilitate the raising of the engines, boilers, and other weights, because it has been ascertained that the tendency of ships to roll has been reduced by this means. Generally speaking, it may be with confidence stated that the centre of gravity of an ironclad can be placed at any requisite height within moderate limits, whether she have one bottom only or two. Next, he supposed the outer bottom of the 'Captain' removed. One effect of this would be to raise her absolute centre of gravity considerably, and another would be to immerse her at least two feet more in the water, and to reduce her freeboard by that amount. He need not say that under such conditions the 'Captain' would have been a much worse ship than she actually was. The truth is, no real objection exists to the double bottom in large ironclads; on the contrary, it possesses immense advantages. Only those who have some fantastic notions on the subject, and who are incapable of grasping scientific problems as a whole, cling to such fallacies as these.

The fallacies connected with guns, which Mr. Reed adverted to, led, in his view of the matter, to serious disadvantages. He considered that any and every argument which led to the use in the present day of bronze as a material of gun manufacture was fallacious; and he discussed the reasons for believing that our service-guns were deficient in twist of rifling, owing to the use of the stud system of giving rotation, and that consequently long projectiles, which would be very useful for naval purposes, could not be fired from them.

[E. J. R.]

WEEKLY EVENING MEETING,

Friday, February 17, 1871.

WARREN DE LA RUE, Esq. D.C.L. F.R.S. Vice-President, in the Chair.

JAMES N. DOUGLASS, Esq.

ENGINEER OF THE TRINITY HOUSE,

On the Wolf Rock Lighthouse.

THE Wolf is a rugged rock, overflowed by the sea at high water, and situated about nine miles south-west of the Land's End. It is about 175 feet in length, and 150 feet in breadth at low water of spring tides. Its highest part is 17 feet above low water, and is covered 2 feet at high water; it is composed of a hard, dark, felspathic porphyry. The surface is rugged, rendering a landing upon it at all times difficult. The depth of water close to the rock is about 20 fathoms

on all sides, except towards the south-east, where a shoal extends for a considerable distance, having only 4½ to 5 fathoms on it at low water, at a distance of a cable's length from the Wolf. At a distance of 1 mile from the rock the depth of water on this reef is about 14 fathoms, but in every other direction around the Wolf the depth is not less than 34 fathoms.

Between the periods of high water and the following low water, the tidal stream runs south-easterly, southerly, and south-westerly, whereas from low water to the succeeding high water it sets north-westerly, northerly, and north-easterly. This peculiarity is supposed to extend to a radius of about 4 leagues from the rock. Situated as the Wolf Rock is, in deep water, and exposed to the full force of the Atlantic Ocean, a terrific sea falls upon it, as may easily be surmised, and of which there are abundant proofs.

The term "Wolf" appears to be quite modern; it is spelt on old maps of the time of Queen Elizabeth and of Charles II. the "Gulph" Rock. It is currently reported in Cornwall that an attempt was once made to plant on its summit the figure of an enormous wolf, constructed of copper, hollow within, and so constructed that the mouth receiving the blasts of the gale should emit a loud hoarse sound, to warn the seaman of his peril. The project, however, was rendered abortive by the violence of the elements.

In the year 1795 a stone lighthouse on the Longships, close to the Land's End, and iron beacons on the Wolf and Rundlestone Rocks, were erected under licence granted to the Honourable Corporation of the Trinity House. The beacon on the Wolf consisted of a pole of wrought iron, sunk into the rock, and supported by six wrought-iron stays; the mast and stays being secured in the rock with lead. This beacon and a similar one on the Rundlestone Rock were soon swept away by the violence of the sea.

Stevenson's Tower.—There is reason to suppose that the question of building a lighthouse on this rock was a subject of discussion between the Admiralty and the late Mr. Robert Stevenson, Engineer to the Commissioners of Northern Lighthouses, as early as the year 1823, as a plan exists of a stone structure designed for the purpose, of that date. This was estimated to require fifteen years to build, at a cost of 150,000*l*. The large amount of this estimate was probably the reason of the idea being abandoned and a beacon being proposed instead, which was afterwards erected. The use of steam and the general progress in science and art has reduced the outlay in time and money on such works very considerably since the above date.

During the years 1836 to 1840, the Trinity House erected a second iron beacon, designed by their engineer the late Mr. Jas. Walker; this beacon consisted of a cast-iron cone, 12 feet diameter at the base, and 22 feet high, filled solid with masonry, and surmounted by a mast and ball. The difficulties of the undertaking may be estimated from the

number of hours actually worked on the rock during each season, which were in

Season.							
1836..	..	..	only	..	..	39½	hours.
1837..	..	..	..	..	..	65	„
1838..	..	..	..	..	..	85	„
1839..	..	..	..	..	..	68½	„
1840..	..	..	..	..	..	44½	„
Making a total of ..						302½	

or only 30¼ working days of ten hours each, during the five years. This beacon was executed at a cost of 11,298*l*. The mast, which was of selected English oak, 12 inches in diameter, was carried away during the following winter; this was replaced by one of wrought iron. This mast was again carried away during a storm in 1844. Another mast, larger in diameter, was fixed, having on it a smaller ball. This was also carried away in 1848. A fourth mast of the same dimensions, but with a ball still smaller in diameter, was fixed; this withstood the force of the sea until it was taken down during the progress of the construction of the present lighthouse.

Rundlestone.—The Rundlestone Rock is about 8 miles from the Wolf and one mile from the shore; it is only 17¾ feet in length and 8¾ feet in breadth at low water of spring tides, and the highest part is only 8¼ feet above low water. The only available space for a beacon on this rock is about 4 feet square. The rock, which is composed of hard grey granite, forms part of a very dangerous group of shoals. A second iron beacon was erected on this rock under great difficulty by my father in the years 1841 to 1843. The iron mast of this beacon was carried away during a storm in October, 1844. In the following January, and before any repairs could be effected, the cone was struck by a vessel and torn away. The vessel, seriously damaged, fortunately succeeded in getting into the port of Hayle. The beacon was reinstated during the following summer, when the globe was reduced in size. This held on until the winter of 1854, when it was carried away during a heavy storm. In the year 1856 it was again reinstated under my superintendence, but during a severe storm in the succeeding winter it was again carried away. This danger is now efficiently guarded by an iron bell buoy, designed by me for the Trinity House. The bell, weighing 3 cwt., is fixed on a wrought-iron stand attached to the deck, and is sounded by four long pendulum clappers, which are Y-shaped, thus having two points of suspension, and rendering the use of the ordinary guides unnecessary. The length of the swing of the clappers is limited by india-rubber buffers. The buoy is constructed with a central cylindrical water-tight compartment large enough to float the buoy in the event of a vessel running foul of it and driving in the outer plating. Several of these buoys are now in use and some have been seriously damaged, but in no instance has one of these been sunk.

The success which had attended the efforts of the Trinity House in the erection of lighthouses on the Bishop Rock, Scilly; the Smalls Rock, at the entrance of the Bristol Channel; and the Hanois Rock, at the west end of Guernsey, induced the Corporation to undertake the erection of a lighthouse upon this dangerous rock; and in the year 1860, the late Mr. James Walker, their engineer, was instructed to furnish a design for and an approximate estimate of the cost of the work. These having been approved, I, being at the time engaged in completing a similar structure on the Smalls Rock, was appointed to carry out the work as resident engineer. On the 1st July, 1861, I made the preliminary survey of the rock for the purpose of determining the exact position of the proposed tower. The weather had been very unpropitious, and on this the only opportunity which had occurred for leaving my work at the Smalls, I landed upon the rock, and made the best use I could of the short time which the state of the tide allowed, but the sea getting up meanwhile put a stop to my work; and as a boat could not, from the increased swell, approach the rock with safety, I was hauled on board through the surf by a line fastened round my waist. This mode of embarking was frequently resorted to afterwards for getting the workmen off the rock when caught by a sudden change of weather and increase of surf.

In the autumn of 1861, a workyard was taken at Penzance, 17 miles from the Wolf, and arrangements were made for commencing operations at the rock in the following spring. The vessels fitted out for the work consisted of a steam-tender of 60 horse-power, five barges for conveying the stone to the rock, each of 40 tons burden, and a schooner of 100 tons burden, the latter being specially fitted as a barrack for the workmen when afloat.

The form of the tower is somewhat similar to Smeaton's celebrated Eddystone, but is of much larger dimensions; it differs but little from those of the Bishop, Smalls, and Hanois. The stepped outline of base was first adopted by Mr. Walker for the Menai Lighthouse in the year 1838. This stepping affords additional weight at the base of the tower, besides affording protection to the horizontal joints in the masonry; and the vertical face of each step has a tendency to check the upward flow of heavy seas on the tower. Its exact height is 116 feet $4\frac{3}{4}$ inches, its diameter at the base 41 feet 8 inches, and near the top, at the springing of the curve of the Cavetto under the lantern gallery, the diameter is 17 feet. For a height of 39 feet $4\frac{1}{2}$ inches from the base, the work is solid, with the exception of a space forming a tank for fresh water. At the level of the entrance door the walls are 7 feet $9\frac{1}{2}$ inches thick, whence they gradually decrease throughout the whole height of the shaft to 2 feet 3 inches at the thinnest part near the top. The shaft of the tower is a concave elliptic frustrum, the generating curve of which has a major axis of 236 feet, and a minor axis of 40 feet. It contains 44,506 cubic feet of granite, weighing about 3296 tons; and its centre of gravity is 36 feet $2\frac{1}{4}$ inches above the base.

Dovetailing.—In consideration of the exposed position of the work, it was determined to dovetail each face stone both vertically and horizontally, in accordance with the system suggested by my father, and first adopted at the Hanois Lighthouse, Guernsey. It consists in having a raised dovetailed band 3 inches in height, on the top bed and one end joint of each stone; a corresponding dovetailed recess is cut in the bottom bed and end joint of the adjoining stones, with just sufficient clearance for the raised band to enter it freely in setting; immediately the cement has hardened, the stone is so firmly secured to the adjoining stones as to be nearly equal in strength to solid granite. This system of dovetailing also affords great protection to both horizontal and vertical joints against the wash of the sea when the work is first set.

An ingenious system of dovetailing and cramping was adopted by Smeaton at the Eddystone Lighthouse. The chief defect that was found in this was the want of a perfect water-stop in the horizontal joints. An improved system of slate joggling, also very ingenious, was adopted by the late Mr. Walker. This was found to be rather complicated in practice, and from the number of pieces required for securing a single stone, the work of setting the masonry was retarded. In addition to the security afforded by the dovetailing, each stone of the first and second courses was secured to the rock by strong bolts of yellow metal, and each face stone of the third to twentieth courses inclusive was secured to the course below in the same manner. For the inside stones galvanized steel bolts were used. All the holes for the bolts were bored in the workyard; and so accurately was this executed, that no instance occurred where the lower part of a hole was found to be out of position for properly inserting and wedging up the bolt at the rock. The masonry to the level of high-water spring tides was set in fresh Medina Roman cement, mixed with an equal portion of clean sharp sand; the remainder of the work was set with the best Portland cement, mixed with sand in the same proportion.

In spite of the precautions taken for the security of the stones in the lower portion of the building, thirty-four stones of the 5th course (which course it was found impossible to complete at the end of the season of 1865) were carried away during a heavy storm which raged on the 24th and 25th of November of that year. It is supposed that the damage was caused by a floating wreck, as marks of the cutting of a chain over the edge of the masonry were observed when an inspection could be made. It was also ascertained, soon after the storm, that the ship 'Star of England,' of 1544 tons register, from Calcutta, bound to London, was nearly lost there during the storm; and the vessel, which was said to have been within 100 yards of the rock on the weather side, was only canted clear of it by cutting away her mizen-mast. It was therefore concluded that this mast with its gear must have driven against the masonry, and caused the damage.

The general internal arrangements of the tower are as follows. The lower room, which is approached by a vertical gun-metal ladder on the outside of the tower, is called the entrance chamber; in it are stowed the ropes, life-buoys, and other appliances for landing at the rock: the second room is used for the stowage of coals, firewood, &c.; the third room, called the store-room, is fitted with a crane for hoisting the coals, water, oil, provisions, &c., from the rock, and in it are stowed the stores and provisions of the light-keepers; in the fourth, or oil room, is stowed the colza oil for the service of the light. The fifth, or living room, is fitted with a convenient cooking-range, &c.; it has also a bookcase for the circulating library provided by the Trinity House, which is changed every three months. The sixth, or bed-room, is fitted with five bed-berths, &c. The seventh, or service room, is fitted with a stove for keeping up the temperature of the lantern during cold weather, and closets for storing everything necessary for the service of the light; the windows of this room are specially arranged for admitting air to the lantern, and for regulating the ventilation in all states of the weather. The supply of air is admitted by a valve at the upper part of the window so as to pass above the head of the light-keeper on duty, and upwards through an iron grating surrounding the lantern floor. The step-ladders, for ascending from floor to floor, and the partitions between the rooms and staircase are of cast iron, and the use of wood for the fittings has been limited as much as possible, as a precaution in case of fire. The doors, windows, and storm shutters are constructed of gun-metal.

Lanterns. — Lighthouse lanterns were formerly invariably constructed with vertical framing and flat glazing, and both framing and glazing were practically as perfect as was required for catoptric lights. But with the more perfect dioptric instruments now in use, the interception by the lantern-framing of the vertically-elongated beam of light sent from these instruments was found to be a matter of importance, and with the view of reducing this interception to a minimum, inclined framing was adopted some years since by the late Mr. Alan Stevenson, engineer to the Commissioners of Northern Lighthouses. These lanterns are glazed with triangular facets of glass. In adopting the cylindrical form for the glazing with helically-curved framing, the greatest possible inclination can be given to the framing, thus reducing the interception of the light to a minimum. The lantern is one of the cylindrical helically-framed type, designed by me and adopted by the Trinity House. The maximum interception of the light sent from a first-order dioptric apparatus for fixed light is $3\frac{1}{2}$ per cent., and the minimum $2\frac{2}{10}$ per cent., giving a mean of $3\frac{1}{5}$ per cent. for the whole illuminated area; indeed, the obstruction is so small that it casts no sensible form of shadow upon a screen at 40 feet from the light. This lantern is framed with puddle steel, the framing is rolled in halves, each of the quadrilateral and triangular panels are welded together separately, and

adjusted to size and curvature on a gauge-block; they are afterwards fitted and riveted together, each pair of quadrilateral panels fishing the junctions of the frames above and below them, and thus forming a rigid framing of nearly uniform sectional area and strength throughout the lantern for the support of the plate-glass. The glazing, which is half-inch thick, is carefully bent to the required curvature and fitted to standard templates, thus ensuring optical accuracy and the perfect fitting of any pane throughout the lantern. With the cylindrical glazing of this lantern the light sent from a dioptric apparatus always falls nearly normally upon the internal surface, consequently the usual scattering of a portion of this light by reflexion with lanterns glazed with flat glass is to a considerable extent prevented. The curvature of the glass also affords additional strength for resisting storms. I have found from actual experiment that this additional strength compared with the ordinary flat glazing amounts to 58 per cent. I would here desire to record the very kind and valuable advice and assistance I received from the late Professor Faraday in conducting the necessary experiments for determining the exact form and arrangement of the framing of these lanterns; also the careful investigation of the subject that was afterwards made for the Trinity House by Professor Tyndall before these lanterns were finally adopted by them.

This lantern was manufactured by Messrs. Hodge and Sons, of Millwall, and was exhibited at the Paris Exhibition of 1867, while the curved plate-glass for glazing it and the dioptric apparatus were manufactured by Messrs. Chance Bros. and Co., of Birmingham. The instrument is probably the most perfect for the purpose that has yet been constructed. With the view of giving the Wolf light a perfectly distinctive character, a revolving dioptric light of the first order, showing alternate flashes of red and white at half-minute intervals, was resolved upon. This arrangement involved the consideration of the important question—which does not appear to have been previously determined with accuracy—of disposing in each beam the relative proportion of light to allow for the loss in the red beams by passing through a ruby glass medium, and producing at all distances at which the light can be seen, with variable states of the atmosphere, flashes of nearly the same strength. The investigation of the subject was entered into by Professor Tyndall, the scientific adviser of the Trinity House; and as it was one which could not be determined with accuracy by photometric measurements, a visit was made by Dr. Tyndall and myself to the Rock Lighthouse, near Liverpool, which has a catoptric revolving light, showing one red flash succeeded by two of white, at intervals of one minute; and inquiries and observations were made by us on this light at the Point of Air Lighthouse, at a distance of 11 miles, and at the Great Ormes Head Lighthouse, at a distance of 30 miles. Experiments with red and white lights were also made at the experimental lighthouse of the Trinity House at Blackwall, and observations on these were taken from a station at Charlton, at a

distance of 2 miles. From these practical tests it was determined that the quantity of light to be appropriated to the red beams should be to that of the white in the ratio of 5275 to 2250, or as 21 to 9 nearly. The apparatus has sixteen panels of refractors and lower prisms and eight panels of upper prisms to the circle. Eight panels of refractors and lower prisms of 18° each are appropriated to eight beams of white light; and eight panels of refractors and lower prisms of 27° each, together with the eight panels of the upper prisms of 45° each, to eight beams of red light. The colour is produced by ruby glass placed in front of the panels, and revolving with the apparatus. The illuminating power of each beam sent from the apparatus is estimated at 31,500 English candles or units of light.

The optical portions of the apparatus were designed by Mr. James T. Chance, to whose scientific attainments is, in a great measure, due the present excellence of manufacture in this country of the illuminating apparatus of our lighthouses.

[Here was exhibited a revolving dioptric light of the fourth order, kindly lent by Messrs. Wilkins and Co., Lighthouse Engineers, of Long Acre. This apparatus had twelve lenticular panels gathering up the continuous sheet of light into twelve divergent beams. Mr. Douglass coloured six of these beams by ruby glass shades in the manner adopted for the Wolf apparatus, but each beam, having the same initial intensity, the power of the red beams was considerably reduced by their passage through the colouring medium.]

Fog-bell.—A fog-bell weighing 5 cwt. is fixed on the lantern gallery; it is struck by two hammers, worked by machinery, fixed in the pedestal of the illuminating apparatus, but independent of that for rotating the latter. For the purpose of giving the signal a distinctive character for the station, the machinery is arranged for striking the bell three blows in quick succession, at intervals of fifteen seconds.

Landing Platform.—In consideration of the great difficulty experienced in landing upon the Wolf, which can only be effected on the north-east side, and even there the surface is rugged and without any vertical face for a boat to approach, it was determined to construct a landing platform of masonry. Frequent tides, which did not ebb low enough to admit of working at the foundation pit for the tower, were worked at this platform; and so rapidly did this portion of the work progress that the platform was nearly completed before the foundation pit could be prepared for setting the first stone of the tower. This platform greatly facilitated the erection of the lighthouse, and will prove of great value hereafter, from the convenience it will afford for landing and embarking at times when it would be impossible to do so without it. The landing platform contains 14,564 cubic feet of masonry, making together with the tower a total of 59,070 cubic feet, or about 4375 tons.

Working Season of 1862.

On the 17th March, the working party got upon the rock, and began to cut out the foundation pit. The insecurity of the foothold, and the constant breaking of the surf over it, rendered great precaution necessary for the safety of the workmen. Heavy iron stanchions were sunk into the rock around the site for the foundation, and each man worked with a safety-rope lying near him, one end of which was attached to the nearest stanchion. An experienced man was always stationed on the summit as "crow," to look out for the sea, who would give warning of such waves as were likely to sweep the rock, when the men would hold on, head to the sea, while it washed over them; picks, hammers, and jumpers, some exceeding 20 lbs. in weight, were frequently found to have been washed away, when the waves had passed and were followed by a lull.

Blasting.—The danger was further increased by frequent blastings of the rock with gunpowder; the only shelter that could be obtained for protection from the showers of falling pieces of rock with each blast, was by crowding all hands together under a temporary penthouse hastily formed around the iron beacon each time we landed. Three or four lucky individuals, more nimble than the rest, usually contrived to scramble through a man-hole into the upper part of the cone of the iron beacon.

These circumstances are not related to magnify the difficulties of the work, but in order that attention may be directed to the precautions taken, and to the fact that they were successful in preserving life.

On the 29th September, the last tide of the season was worked; only twenty-two landings had been effected and eighty-three hours of work obtained on the rock for the season, although not a single opportunity had been lost when it was possible to work even half an hour. The season was altogether a very unfavourable one for such an undertaking. During these eighty-three hours considerable progress was made in blasting and cutting out the foundation pit for the tower and in the erection of the landing platform.

On the death, in October, 1862, of the late Mr. James Walker, the Engineer to the Trinity House, I was appointed by the elder brethren to that office; and my brother, who was then completing the Hanois Lighthouse, succeeded me as resident engineer at the Wolf.

Season of 1863.

The first landing was effected on the 20th February, and the last on the 24th October. During the season, thirty-nine landings were effected, and the work on the rock was proceeded with during two hundred and six and a half hours. At its close, the cutting of the foundation pit and the erection of the landing platform were about

half executed, and the dressing of the 6th course of masonry in the workyard was completed.

Season of 1864.

The first landing was effected on the 9th April, and the last on the 5th November. During this season forty-two landings were effected, and two hundred and sixty-seven hours' work obtained on the rock; on the 6th August the first stone of the tower was set. At the close of the season, thirty-seven stones of the 1st entire course or 2nd course of the tower were set; the landing platform was nearly completed, and the dressing of the 10th course was finished in the workyard. The iron derrick landing crane was erected on the end of the landing platform; it has a solid wrought-iron mast, fixed in a cast-iron well, into which the machinery, when not in use, is lowered by a rack and pinion, and is then secured by strong, wrought iron hinged covers. The wrought-iron derrick, when not in use, is lowered into the long protecting chamber, and is secured therein with strong iron covers.

The barges for conveying the stones were specially fitted for the work. The hold of these barges is fitted with rollers, on which the stones are stowed in the exact order in which they are required for the work. Each stone, as required to be landed, is rolled on to one of the stone-trucks at the stern of the barge, and is drawn up to the level of the deck by a chain led from the winch on the deck; the chain from the landing crane is then shackled to the lewis fixed in the stone; the single block of a strong rope veering tackle is also attached to the lewis; one end of this tackle is secured to one of the windlass bitts, and the other end to a break barrel on the winch. As the chain of the landing crane draws the stone from the truck over the roller at the stern of the barge, with the heave of the vessel the veering tackle is eased away by the break, and the tackle is kept just sufficiently taut to prevent the stone being driven by the sea against the rock. Twenty to thirty blocks of stone weighing 4 to 5 tons each have frequently been landed in this manner in one day without damage, and with a rise and fall of wave of 12 feet.

Each barge, when at the landing crane, was moored stem and stern, with 10-inch coir hawsers; and the stern hawsers, even of this size, were frequently broken. The barrack schooner for the accommodation of the resident engineer, his assistants, and working party, was moored at a distance of $\frac{3}{4}$ mile from the rock; and remained there as long as there was any opportunity of doing work. When this was no longer possible, and there was no immediate prospect of better weather, the moorings were slipped, and the vessel was taken to Penzance, there to await another opportunity.

The landing boat was designed and constructed expressly for the work; she is built diagonally, of two thicknesses of elm plank, without timbers or floors, and is provided with a deck and landing mast

forward. The deck and gunwale forward are covered with rough rope matting, for the purpose of affording a good foothold in jumping from or into the boat. Each workman is provided with a cork lifebelt, which he is compelled to wear while landing or embarking from the rock; and it was frequently necessary for the safety of the men, that they should wear these belts during the whole of the time that they were engaged upon the rock.

Season of 1865.

The first landing was effected on the 11th April, and the last on the 17th December. Between these dates forty-one landings were effected, and the work on the rock was carried on for two hundred and fifty hours. At the close of the season the 4th course of masonry was completed, and thirty-four stones were set in the 5th course.

In the workyard, the dressing of the 18th course was completed. It was fully expected that the 5th course would have been completed at the rock before the works were closed for the winter; but the state of the weather rendered this impossible, and those stones which were set, thirty-four in number, were carried away, as previously stated, during a severe storm in November.

Season of 1866.

The first landing was effected on the 5th March, and the last on the 13th October. During the summer thirty-one landings were effected, and two hundred and twenty-four hours and a half worked on the rock. At the close of the season, the 9th course of the tower was completed, and two stones were set in the 10th course. In the workyard the dressing of the 26th course was finished. During the season the wrought-iron setting crane was erected on the tower, and used for setting the work. At the close of the season, the bare post of the crane, a wrought-iron cylinder of great strength, was left standing 20 feet above the masonry and 23 feet above high water of spring tides, but during the winter it was broken off flush with the surface of the work.

Season of 1867.

No opportunity occurred for landing on the rock until the 6th May, and the last for the season was on the 5th November. During this time forty landings were effected, and three hundred and thirteen and a half hours worked on the rock.

At the close of the season, the 23rd course was completed and eight stones were set in the 24th course. In the workyard the dressing of the 39th course was completed. At the commencement of this season's operations at the rock, a new mast for the setting crane was fixed, a topmast was also fitted to it, and the hoisting and

setting of this portion of the work was carried out. On the 31st June, the sea being very smooth, all hands were able to remain on the rock for the first time over high water, on the top of the 11th course.

Season of 1868.

Between the first landing on the 31st March, and the last on the 14th October, thirty landings were effected, and two hundred and seventy-six and a third hours worked on the rock. At the close of the season, the 47th course of the tower was completed, and in the work-yard the dressing of the masonry was finished. On the 17th and 18th June the steam-winch with its boiler was fixed in the first room of the tower, and on the 29th was used for the first time; this was doubtless the first occasion of the employment of a steam-engine upon a tidal rock. It worked successfully to the completion of the tower, and considerably facilitated the process of construction. The average time occupied in raising each block of stone from the landing platform to the top of the work was only two and a half minutes, while to perform the same duty by manual labour would have required fifteen minutes.

Season of 1869.

The first landing was effected on the 16th March, and on the 19th July the last stone of the tower was laid by Sir Frederick Arrow, the Deputy-master of the Trinity House. To this date there had been twenty-one landings and one hundred and ninety-four and a half hours of work on the rock, making a total for the eight working seasons of two hundred and sixty-six landings, and eight hundred and nine and a half hours, being only one hundred and one working days, of ten hours each, for the erection of the tower.

Lighting-up.—Towards the end of the year, the tower was got ready for the exhibition of the light which was advertised for the 1st of January, 1870; but Christmas approached, and no opportunity had occurred for some time for communicating with the workmen in the tower, except by signal. I was at Penzance waiting for an opportunity to go off with the resident engineer of the work and the three light-keepers appointed to take charge of the tower. On Christmas morning the opportunity so anxiously looked for arrived, when we proceeded in the steam-tender, and succeeded in effecting a landing; but before the effects of the light-keepers were fairly ashore, and my inspection could be made of the works in the tower, the sea had increased so much that the landing boat could no longer approach the rock. We had, therefore, with a cold north-east wind blowing, to be hauled one at a time through the surf to the boat, which we all preferred to the prospect of a lodging for an indefinite period in the lighthouse.

Since the completion of the tower, the heaviest seas that have been experienced were on the 11th September, 1869, during a violent gale from the westward, when large quantities of water went over the tower; but the residents state that although the shock was distinctly felt with each wave-stroke, scarcely any tremor was perceptible.

The light was exhibited on the 1st January, 1870, and has since been continued with regularity every night from sunset to sunrise. Before the light could be shown, the services of my brother, the resident engineer, were required for the erection of a similar work, on the great Basses Rock, off the south-east coast of Ceylon; the charge of the completion of the work was therefore entrusted to Mr. M. Beazeley, the assistant-engineer at the work, who is now proceeding with the erection of a new lighthouse upon the Longships.

It is a source of thankfulness to me to be able to state that this difficult and dangerous work has been brought to a successful termination without loss of life or limb to any person employed. This success is mainly due to the steady perseverance of the resident engineer, his assistants, and their workmen; and I would desire to record the personal interest and willing heartiness exhibited by those employed upon this and similar works that have been carried out by the Trinity House.

As this is a matter of the utmost importance, affecting as it does the success of similar undertakings, a somewhat detailed account of the system under which the workmen are employed may not be considered uninteresting. Each workman is engaged at a daily rate of wages at the commencement of each season, and signs an agreement for one year; the wages are paid monthly, from which is deducted a sum equal to 2*d.* per day, as a reserve until the end of the season, which reserve is forfeited in the event of the man leaving the employment without giving a month's notice. When at sea an allowance of 25 per cent. is added to the ordinary wages, and as a further encouragement for good conduct and perseverance, a gratuity of 6*d.* is allowed for every hour worked upon the rock. This gratuity is not payable, however, in the case of misconduct, or in the event of the man leaving the employment during the working season. The sum of 1*d.* per day is deducted from the wages of each workman for a sick and accidental insurance fund, which is managed by a committee of four of the workmen, elected by the whole of the subscribers, the resident engineer acting as treasurer.

In case of sickness or accident, each subscriber is paid 2*s.* per day, and is provided with a medical attendant. At the close of the work a balance of 48*l.* 8*s.* 8½*d.* remained in the hands of the treasurer, which was distributed amongst six persons connected with the works, *viz.* two who were then on the sick-list, three who had met with accidents on the works, and the widow of a workman who had recently died.

The average number of persons of the various classes employed at the Wolf Lighthouse was:—

Resident Engineer	1	Engineers of Steam-tug	2
Assistant ditto	1	Firemen of ditto	2
Clerk	1	Millwright	1
Storekeeper	1	Masons	22
Foreman Mason	1	Carpenters and Shipwrights	6
Master of Steam-tug	1	Smiths	6
Mate of ditto	1	Seamen	14
Master of Sailing Tender	1	Miners	2
Mate of ditto	1	Labourers	6

TOTAL, 70.

The total cost of the undertaking, including lantern, illuminating apparatus, cost of workyard, vessels, and all incidental expenses, may be taken at 62,726*l*. This cost, considering the exceptional difficulties of the work, compares favourably with any similar work yet executed.

[J. N. D.]

WEEKLY EVENING MEETING,

Friday, February 24, 1871.

WILLIAM SPOTTISWOODE, Esq. Treasurer R.S. & R.I. and Vice-President, in the Chair.

W. MATTIEU WILLIAMS, F.C.S.

On Rumford's Scientific Discoveries.

THE speaker ventured to assert that it would be difficult, or even impossible, to name any philosopher, statesman, and philanthropist of equal eminence to Benjamin Thompson, Count of Rumford, concerning whom so little is popularly known. This may be explained by the facts that his statesmanship and philanthropic efforts were chiefly exerted abroad, and that his philosophical researches resulted in scarcely anything admitting of attractive lecture-table illustration, while the discoveries of so many of his contemporaries and immediate successors were most brilliant in this respect.

The electrical machine, the voltaic battery, oxygen, hydrogen, chlorine, the metals of the alkalis, &c., by supplying lecture-table experiments of such marvellous and previously unknown brilliancy, developed a new race of teachers—the popular experimental lecturers, to whom we are mainly indebted for the popular diffusion of scientific knowledge during the present century, and the philosophers whose names are connected with the discovery of the materials which supplied these teachers with their favourite illustrations, are naturally the best known to their pupils.

The researches of Rumford are nevertheless especially worthy of general attention, as his subjects literally come home to all of us, the greater part of his life having been devoted to studying and applying the philosophy of common things; he may be, in fact, regarded above all others as the philosopher of common things, the science of feeding, clothing, warming, and sheltering of mankind having been his chief pursuit, as the following list of the subjects of his essays and researches will show.

The Force of Fired Gunpowder.—Improvements in Field Artillery and Guns generally.—Naval Architecture.—Naval Signals (the essays on these two last-named subjects have not yet been published).—The Transmission of Heat through Solids, Liquids, Gases, and Vacuo.—The Relative Warmth of Different Materials used for Clothing.—The Quantities of Moisture absorbed from the Atmosphere by Different Materials used for Clothing.—The Production of Air from Water.—Photometry.—Coloured Shadows.—The Harmony of Colours.—The Chemical Properties which have been attributed to Light.—The Weight or Ponderability ascribed to Heat.—The Suppression of Mendicity, and the Economical Clothing, Feeding, Housing, and Industrial Education of the Poor.—The Philosophy and Practice of Cookery.—The Nutritive Value of Different Kinds of Food.—The Cultivation and Use of the Potato.—The Construction of Stoves and Open Fireplaces.—The Curing of Smoky Chimneys.—The Organization of Military Academies.—General Military Organization.—The Education and the Civil Utilizing of the Soldier.—The Economy of Fuel.—The Construction of Kitchen Furnaces—of Boilers—Roasters—Saucepans—Stewpans—Gridirons—Dripping-pans—Ironing Stoves—Cottage Fireplaces—Steam Stoves—Limekilns—Ovens—Teakettles—Steam Cooking Apparatus—Dye-house Vats—Distillers' Coppers—Drying-houses—Laundries—Lamps, Safety-valves, &c.—The Preparation of Cheap Soups, Indian Puddings, Macaroni, Potato Salads, Potato Dumplings, &c.—The Internal Movements of the Particles of Heated Fluids.—The Final Cause of the Saltness of the Sea.—The Influence of Water in Equalizing Climate.—The Heating of Buildings by Steam.—The Boiling of Water and various Solutions by the Latent Heat of Steam.—The Salubrity of Warm Bathing (an Essay advocating the introduction of the Turkish Bath).—The Pleasure of Eating, and the Means that may be employed for Increasing it.—The Tensile Strength of Different Substances.—The Suppression of Usury in Bavaria.—The Improvement of the Breed of Horses and Horned Cattle in Bavaria—The Relative Heating Power of Coal and Different Kinds of Wood.—The Source of Heat excited by Friction.

This list is mainly made up of practical subjects, on which Count Rumford did not merely write suggestive or speculative essays, but which he dealt with practically by carrying out successfully on a practical scale his suggestions and improvements. All his practical successes were effected by the application of scientific principles, Rumford's method invariably being to set before himself the task to be

performed, then to determine the philosophical principles involved and having done this, to apply these principles practically. Most of his purely philosophical investigations were suggested by demands thus practically created, or by phenomena observed in the course of his practical work.

The great characteristic of Rumford's whole career is that *all* his practical work was strictly philosophical, and most of his philosophical work was eminently and directly practical.

When, for example, he was commissioned by the Elector of Bavaria to reorganize the Bavarian army, he studied the philosophy of cookery, and made experiments upon the nutritive value of different kinds of food, in order that he might feed the men as well and as economically as possible. His improvements in stoves, kitchen utensils, &c., his numerous researches and essays on subjects connected with the general economy of the kitchen and the preparation of food, are the results of his thorough and philosophical method of solving this very practical problem. The barrack requirements led him to study further the economy of fuel in the warming of dwellings, the construction of heating and ventilating apparatus, and of buildings generally.

In order to determine the best material for the soldier's clothing, he first considered the function of clothing, and determined that in winter it should act by resisting the transmission of the animal heat to the cooler atmosphere, and thereby maintain the body at the temperature required; that for this purpose a non-conductor or a bad conductor of heat is required. The relative conducting power of different clothing materials being in his time unknown, he constructed a theoretical soldier in the form of a thermometer, which he could clothe with the materials to be tested. By heating this clothed thermometer, and allowing it to cool in an apartment of constant temperature, he obtained the following results:—

Thermometer surrounded by	Air	.. ..	cooled from 70° to 10° (Reaumur)	in 576 seconds.			
„	16 grs. of	Fine lint	„	„	1032	„	
„	„	Cotton wool	„	„	1046	„	
„	„	Sheep's wool	„	„	1118	„	
„	„	Raw silk	„	„	1284	„	
„	„	Beaver's fur	„	„	1296	„	
„	„	Eider-down	„	„	1305	„	
„	„	Hare's fur	„	„	1315	„	

Finding that the raw silk only occupied $\frac{1}{55}$th of the whole bulk of the outer bulb containing it, he calculated that if it were an absolute non-conductor, it should, as compared with the envelop of air contained in the same space, only increase the resistance by ten seconds, but the experiment showed that it had above seventy times the effect. Therefore the resistance of the silk fibre cannot account for the result. Connecting this with his previous investigations on the convection of heat by liquids, Rumford inferred that air is a non-conductor of heat, that the fibres acted by enclosing the air between them, and preventing

those convection movements by which alone it can carry heat away from a body in contact with it. He confirmed this explanation by a series of experiments. He found that when the thermometer was surrounded by the same quantity of the above-named materials more closely twisted or woven, so as to leave less air space between the fibres, their power of resisting the passage of heat was diminished proportionately to the closeness of the weaving, or twisting, or packing.

He thus not only solved his practical question of clothing, but made important discoveries respecting the laws of transmission of heat by gases, and further applied these to retaining heat in furnaces and buildings by means of cellular walls, or double walls and windows.

The question of what is the best material for summer clothing was decided in like manner. He first considered how the body retains its temperature when exposed to direct summer sunshine in hot climates, or otherwise, when a thermometer similarly exposed rises above blood heat. He concluded that it is by the evaporation of the insensible perspiration. How, then, may clothing aid this? Evidently by its power of absorbing the aqueous vapour. Do clothing materials thus absorb vapour? If so, do they differ in their powers of absorption?

To answer these questions he exposed the following substances, carefully cleaned, upon china plates for twenty-four hours, in a room that had for several months been dried by a German stove, its atmosphere having for six hours previous to the experiment been raised to 85° Fahr. After this exposure 1000 parts of each were weighed in the dry room; then this quantity was exposed for forty-eight hours in an uninhabited room and weighed again; then for seventy-two hours in a very damp cellar. The results were as follows:—

1000 Parts of	After 48 hours in room, weighed	After 74 hours in cellar, weighed
Sheep's wool	1084	1163
Beaver's fur	1072	1125
Fur of Russian hare	1065	1115
Eider-down	1067	1112
Silk — Raw	1057	1107
Silk — Ravellings of white taffety	1054	1103
Linen — Fine lint	1046	1102
Linen — Ravellings of fine linen	1044	1082
Cotton wool	1043	1089
Silver wire—Ravellings of gold lace	1000	1000

From these and other experiments he concluded that light flannel is the best clothing for summer, and he strongly advocates its universal adoption. The soundness of his conclusion is confirmed by the subsequent experience both of soldiers and sailors, and of cricketers, rowers, furnace-men, and all who are engaged in work that induces much perspiration.

Count Rumford's researches and inventions connected with his greatest subject, that of cookery, afford abundant examples of his applications of philosophy to practical affairs; but the extent of the subject forbids the citation of more than one small characteristic sample. After advocating the extended use of Indian corn and giving general directions for its use, and particularly for the preparation and cooking of "Indian pudding," including the scientific manipulation of the pudding bag, he concludes as follows:—"The pudding is to be eaten with a knife and fork, beginning at the circumference of the slice and approaching regularly towards the centre, each piece of pudding being taken up with the fork and dipped into the butter, or dipped *in part only*, as is commonly the case, before it is carried to the mouth."

Again in his essay "On the Pleasure of Eating and the Means that may be employed for increasing it," after showing that the pleasure of eating is produced by bringing the surface of the food in contact with the surface of the tongue and the palate, and how a given quantity of food may have its contact surface increased, he says in apology for his subject, "If a glutton can be made to gormandize two hours upon two ounces of meat, it is certainly better for him than to give himself an indigestion by eating two pounds in the same time."

As an example of his theoretical work his speculations on the movements of the molecules of a heated fluid may be cited. He supposes the case of a coin or pebble at the bottom of an open vessel of water, and says, "As a ray of light cannot fail to generate heat when and where it is stopped and absorbed, the rays which, entering the water and passing through it, impinge against the small solid opaque body at the bottom of the vessel are there absorbed, and must necessarily generate a certain quantity of heat; a part of which will be communicated to those colder particles of water which repose on its surface."

He had already shown by a series of very simple and conclusive experiments that the particles of a fluid thus heated are set in *visible* motion, and he supposes the case of a molecule of water in contact with the coin, and having a diameter of $\frac{1}{000000}$th of an inch to move at the least rate of visible motion $\frac{1}{100}$th of an inch per second. Thus it will move through 10,000 times its own diameter in a second. Taking the greatest velocity of a 9-pounder cannon ball, 4 inches in diameter, at 1600 feet per second, it passes through only 4800 diameters per second; thus the *relative* velocity of the molecule of water is mo e than double that of the cannon ball. Such a molecule thus moving will come in contact with at least 600,000 others per second, to each of which it will impart a portion of its heat and motion. Thus, every inequality of temperature induces violent molecular agitation, and Rumford asks whether animal life may not depend upon such motion, seeing that the organic apparatus is mainly fluid, or fluids contained between walls and in channels, and that there is a continual internal heating and external cooling due to respiration and surface evapo-

ration and radiation, or in his own words: "Do not respiration, digestion, and insensible perspiration all tend evidently (that is to say, according to our assumed principles with regard to the manner in which heat is propagated in fluids) to *produce* and to *perpetuate* this inequality of heat in the animal fluids. And do we not see what an immediate and powerful effect they have in increasing the intensity of the action of the powers of life?"

Intimately connected with this essay is another, which appears to be worthy of more attention than it has received, especially in connection with the modern growth of our knowledge of the transmission of light and heat in gases and liquids, and the conversion of one force into another. The title of this essay is "An Inquiry concerning the Chemical Properties that have been attributed to Light." Rumford made a number of experiments upon compounds of gold and silver, by which he showed that the decompositions usually attributed to light could all be effected in precisely the same manner and degree by obscure heat, a temperature below the boiling point of water being sufficient when the salts were in aqueous solution, or a higher temperature when the dry salts were used. Also that clear solutions of chloride of gold or nitrate of silver are not decomposed by sunlight unless some body capable of absorbing the light, and thereby converting it into heat, is immersed in them. He asks what is the temperature induced upon the absolute surface of such a light-absorbing body at the instant of absorption or conversion of light into heat. He thinks that it must far exceed that indicated by any form of thermometer, because the thermometer only shows the *mean* temperature of the whole mass of the contents of its bulb, and its absolute surface is perpetually parting with its heat by conduction, radiation, and the convection of the surrounding medium. If the single molecule of water above referred to comes in contact with 600,000 others in the course of a second, and communicates some of its heat to each, we cannot ascertain its initial maximum temperature, and even this is below that of the surface of the coin or pebble from which it received its heat by contact.

The time did not permit the speaker to develop these speculations and connect them with modern philosophy as intended, and he therefore passed on to Rumford's celebrated experiments on "The Source of Heat excited by Friction."

These experiments are so well known that it is unnecessary to repeat their details here. It will be remembered that after taking every precaution to insulate his apparatus from any communication of heat from without, and ascertaining that the specific heat of the borings was unchanged, Rumford found that the heat evolved by the friction of a blunt borer against the inside of a metal cylinder was sufficient to boil 18·77 lbs. of water in $2\frac{1}{2}$ hours, and to keep it boiling as long as the friction was sustained. His own conclusion is thus expressed: "It is hardly necessary to add that anything which an insulated body, or system of bodies, can continue to furnish *without*

limitation cannot possibly be a *material substance;* and it appears to me extremely difficult, if not quite impossible, to form any distinct idea of anything capable of being excited and communicated in these experiments except it be MOTION."

The italics and capitals are Count Rumford's. As Dr. Tyndall says, "Rumford in this memoir annihilates the material theory of Heat. Nothing more powerful on the subject has since been written," and "hardly anything more conclusive has since been adduced in the way of establishing that Heat is what Rumford considered it to be—'MOTION.'"

This memoir, and the one immediately connected with it, "On the Weight or Ponderability ascribed to Heat," and his investigations on the Transmission of Heat through Gases and Liquids, are perhaps the most important of Rumford's purely philosophical works, and he is the father of the Royal Institution. All who have followed the recent progress of physical science must admit that this Institution has been a dutiful child and has worthily cultivated its patrimonial inheritance.

Old-fashioned story-tellers were wont to conclude their discourse with a "moral." This old fashion may be profitably followed in the present instance, when we remember that Benjamin Thompson began life in extreme poverty, was a poor teacher in a poor colonial village school, that step by step he rose to such honour and distinction, that when Bavaria was so sorely threatened that its sovereign was obliged to fly from Munich, full powers of temporary sovereignty were placed in Rumford's hands, and he wielded this power with complete success. He practically solved great social problems and achieved great social reforms, such as to this day, after a lapse of above seventy years, we all desire to see repeated, and yet cannot achieve again. He abolished mendicity from a country where it prevailed to an almost unprecedented extent; he succeeded in making the rogues and vagabonds of Bavaria pay all the expenses of their food, clothing, and lodging, and leave a handsome balance towards the maintenance of the police who apprehended them. He thus provided for the poor of an excessively pauperized country without any poor's-rates. He was a great statesman, a practical soldier, the greatest of practical military reformers, a skilful mechanic and engineer, and a successful philanthropist, besides being a distinguished philosopher. All his success is clearly traceable to the fact that whatever he did, from the eating of a slice of pudding to the dictatorship of a nation, was done by rigidly obeying those principles of inductive reasoning which have led to the marvellous triumphs of modern science.

If, therefore, you would make your son a successful soldier, a successful lawyer, a successful statesman, successful in any business or profession whatever, you should give him a sound, practical scientific education; let him learn how to observe and investigate facs, to generalize them, and from such inductions to deduce sound rules for practical conduct.

Modern science affords the best, the highest, and the most useful school of intellectual culture: the great business of the present day is to give to science that decided educational precedence to which it is entitled, and the whole career of Benjamin Thompson, Count Rumford, affords a striking example of the kind of intellectual results we may expect to obtain when sound scientific knowledge and training are afforded to every human being—male and female.

[W. M. W.]

WEEKLY EVENING MEETING,

March 3, 1871,

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President in the Chair.

CAPT. NOBLE, F.R.S.

On the Pressure of Fired Gunpowder.

[Abstract deferred.]

GENERAL MONTHLY MEETING,

Monday, March 6, 1871.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

William Blenkin, Esq.
John Browning, Esq.
Edward Maynard Denny, Esq.
Frederick A. Eck, Esq.
Sir Frederick Elliot, K.C.M.G.
Colonel Augustus Lane Fox.
Paul Graham, Esq.
Col. James Augustus Grant, C.B.
Edward Walter Grubbe, Esq.
George Harcourt, M.D.
Captain F. Helbert.
George William Henderson, Esq.
George Middleton Keill, Esq.
John Kennedy, M.D.
James Macauley, Esq.
Kenneth Robert Murchison, Esq.
Mrs. Sheffield Neave.
George W. Royston Pigott, M.A.
Eustratios Ralli, Esq.
Francis Savage Reilly, Esq.
William Chandler Roberts, Esq.
W. Dehague Routh, Esq.
Mrs. W. Castle Smith,
Thomas Sowerby, Esq.

were *elected* Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Actuaries, Institute of—Journal, No. 72. 8vo. 1871.
American Association—Proceedings of Eighteenth Meeting, Aug. 1869. 8vo. 1870.
Asiatic Society, Royal—Journal, Vol. V. Part 1. 8vo. 1870.
Asiatic Society of Bengal—Journal, No. 165. 8vo. 1870.
Astronomical Society, Royal—Proceedings, Vol. XXXI. No. 3. 1871.
Ballard, Edward, M.D. (the Author)—Typhoid Fever in Islington traced to Impure Milk. (K 98) 8vo. 1871.
Bennett, John B. Esq. Registrar-General, Wellington—Statistics of New Zealand. 1869. fol.
British Architects, Royal Institute of—Sessional Papers, 1870–1, No. 4. 4to.
Chemical Society—Journal for Feb. 1871. 8vo.
Comitato Geologico d'Italia—Bollettini, Nos. 11, 12. 8vo. Firenze, 1870.
Editors—Academy for Feb. 1871. 4to.
American Journal of Science, Jan. 1871. 8vo.
Artizan for Feb. 1871. 4to.
Athenæum for Feb. 1871. 4to.
Chemical News for Feb. 1871. 4to.
English Mechanic for Feb. 1871.
Engineer for Feb. 1871. fol.
Food Journal for Feb. 8vo. 1871.
Horological Journal for Feb. 1871. 8vo.
Journal of Gas-Lighting for Feb. 1871. 4to.
Mechanics' Magazine for Feb. 1871. 8vo.
Nature for Feb. 1871. 4to.
Pharmaceutical Journal for Feb. 1871. 8vo.
Photographic News for Feb. 1871. 4to.
Scientific Review for Feb. 1871. fol.
Elliot, Lady—History of India as told by its own Historians. Edited from Papers by Sir H. Elliot, by J. Dowson. Vol. III. 8vo. 1871.
Geological Institute, Imperial, Vienna—Jahrbuch, 1870, No. 3. 8vo.
Verhandlungen, 1870, No. 10. 8vo.
Geological Society—Quarterly Journal, No. 105. 8vo. 1871.
Geological Survey Office—Mineral Statistics. 1869. 8vo.
Memoirs. 2 Parts. 8vo. 1870.
Catalogue of Maps, &c. 8vo.
Jencken, Henry D. Esq. M.R.I.—Compendium of Modern Roman Law. By F. J. Tompkins and H. D. Jencken, 8vo. 1870.
Linnean Society—Journal, No. 150. 8vo. 1871.
London Institution—Journal, No. 3. 8vo. 1871.
Meteorological Office—Quarterly Weather Report, 1869. III. 4to. 1870.
Preussische Akademie der Wissenschaften—Monatsberichte, Dec. 1870. 8vo.
Royal Society of Edinburgh—Transactions, Vol. XXVI. Part 1. 4to. 1870.
Proceedings, 1869–70. 8vo.
Royal Society of London—Proceedings, No. 125. 8vo. 1871.
Transactions, Vol. CLX. Part 2. 4to. 1870.
Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, Feb. 1871. 8vo.

WEEKLY EVENING MEETING,

Friday, March 10, 1871.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

William B. Carpenter, M.D. F.R.S.

On the Latest Scientific Researches in the Mediterranean.

The objects of the 'Porcupine' Expedition of 1870 were: (1) to continue southwards the Physical and Biological exploration of the Deep Sea, from the northern extremity of the Bay of Biscay, at which it had terminated in 1869, to the Strait of Gibraltar; (2) to prosecute similar inquiries in the Western basin of the Mediterranean between the Strait of Gibraltar and Malta; and (3) to determine whether an outward under-current does or does not exist in the Strait, carrying back Mediterranean water into the Atlantic.

These objects were on the whole satisfactorily attained; the only part as to which there was any want of success being the exploration of the Bay of Biscay, which was prevented by unfavourable weather. The Dredgings and Temperature-soundings on the Atlantic coast of Spain and Portugal gave results of very great interest; while the remarkable contrast to these results afforded by the peculiar Temperature-phenomena of the Mediterranean, and by the comparative sterility of its depths, proves of unexpected value in the elucidation of questions of high importance to the Physical Geographer and the Geologist. The existence of an outward under-current in the Strait of Gibraltar has now been placed beyond all doubt; and the determination of the Physical Cause by which the inward surface-current and the outward under-current are alike sustained, has a direct bearing on a much larger question,—that of the General Oceanic Circulation,—propounded in the speaker's previous Discourses (April 9, 1869, and Feb. 11, 1870).

The Mediterranean Sea consists of two distinct basins, which would be entirely separated from each other by an elevation of the bottom to the amount of about 300 fathoms; for this would establish a continuity of land between Italy, Sicily, and the northern coast of Africa; whereby the *eastern* basin, which extends from Malta to the Levant, would be cut off from the *western* basin, which extends from Malta to Gibraltar. A yet smaller elevation than this would

completely cut off the western basin from the Atlantic; for although the narrower part of the Strait of Gibraltar, near its eastern extremity, has a depth exceeding 500 fathoms, there is a gradual shallowing, with increase of width, towards its western extremity; so that where it opens out between Capes Trafalgar and Spartel, a considerable part of it has a depth of less than 100 fathoms, whilst its deepest channel does not exceed 200 fathoms. The bottom slopes westwards, or towards the Atlantic, from this summit-ridge; which thus constitutes a kind of marine "watershed" between the Atlantic and the Mediterranean. The depth of the Western basin approaches 1500 fathoms over a considerable part of its area; sometimes exceeding that amount; whilst that of the Eastern basin approaches 2000 fathoms in many parts, and reaches 2150. Thus such an elevation of the whole area as would convert the Mediterranean into two "self-contained" lakes, whilst somewhat contracting their surface, would leave their depth comparatively little diminished.

The effect of the ridge at the western extremity of the Strait of Gibraltar is to cut off all direct communication between the *deeper* water of the Mediterranean and that of the Atlantic, whilst allowing a communication between their *surface*-waters; but, as will be presently shown, this limited interchange between Atlantic and Mediterranean water affects the constitution of the latter to a depth considerably greater than that of the channel of communication between them.

Density.—The first point of difference between the water of the Mediterranean and that of the Atlantic, is in regard to their respective proportions of *Salt*.

In the course of this Expedition numerous samples of Mediterranean water were collected from various points of the surface, and from various depths, ranging downwards to 1743 fathoms. These samples were almost entirely obtained from the western basin; but the deepest sample was taken from the eastern basin, about sixty miles beyond Malta. The Specific Gravity of each was determined at the time by Hydrometers specially constructed for the purpose, and specimens of the most remarkable were brought home for verification with the Balance. The proportion of Chlorine in each sample was determined by volumetric analysis, which can be readily carried on aboard ship; and from this the entire proportion of Salt may be estimated with considerable exactness. The Physical and the Chemical method of ascertaining the density of each sample gave results which generally accorded very closely. Similar determinations had been previously made in regard to samples of water taken in the Atlantic, both from the surface and from various depths down to 1095 fathoms, during the voyage of the 'Porcupine' from Falmouth to Lisbon.

A comparison of these results leaves no doubt that there is an excess of Salinity in the water of the Mediterranean above that of the Atlantic; but that this excess is kept within very narrow limits.

The range of Specific Gravity in Atlantic water was from a *maxi-*

mum of 1·0269 to a *minimum* of 1·0261; the *mean* of all the observations being 1·0265.

The Maximum occurred in *surface*-water, the Minimum in *bottom*-water.

The proportion of Chlorine was determined by volumetric analysis in 34 samples. Of these samples 12 were of *surface*-water, 12 of *bottom*-water from various depths down to 1095 fathoms, and 10 of *intermediate* water. The results are expressed in grammes per 1000 cubic centimètres of water:—

	Surface-water.	Intermediate water.	Bottom-water.
Average ..	19·94 ..	19·85 ..	19·75
Maximum ..	20·19 ..	19·94 ..	19·98
Minimum ..	19·81 ..	19·70 ..	19·46

It appears from these results that there is a slight excess of Salinity in the *surface*-water of the Atlantic, as had been previously observed by Forchhammer; the excess, however, being so small as not to neutralize the excess of density which the deeper water derives from its lower temperature and from the pressure of the superincumbent column. Five determinations of the Chlorine contained in samples taken at the same spot, from the surface, and from 10, 25, 50, and 100 fathoms, gave the following results:—

Surface	20·013	50 fathoms	19·909
10 fathoms	19·909	100 „	19·805
25 „	19·909		

A comparison of these seems to indicate that the excess of Salinity, being limited to a mere superficial film, is entirely due to evaporation; and the reason why this more concentrated film does not sink, as it does in the Mediterranean, seems to be that its excess of Salinity is so small, that even at the depth of 10 fathoms its effect on Specific Gravity is neutralized by the greater density arising from reduction of Temperature.

The range of Specific Gravity in proper Mediterranean water (excluding samples taken where the influence of the Atlantic inflow was perceptible) was from a *maximum* of 1·0292 to a *minimum* of 1·0268. But there was here so marked a difference in Salinity between *Surface*-water and *Bottom*-water, as indicated alike by Specific Gravity and by Chlorine determinations, that the results of the two series of observations need to be presented separately:—

	Surface-water.		*Bottom-water.*	
	Sp. gr.	Chlorine.	Sp. gr.	Chlorine.
Average ..	1·0278	20·87 ..	1·0285	21·38
Maximum ..	1·0284	21·32 ..	1·0292	21·88
Minimum ..	1·0265	20·70 ..	1·0281	21·08

The *maximum* density having first presented itself at moderate depths in the Western basin, it was expected, when greater depths were sounded, that the bottom-water would prove yet more dense.

This, however, was not found to be the case; for the *greater* the depth, the *less* was the excess in the density of the bottom-water. Thus in the 1743 fathoms' sounding, the Specific Gravity of the Surface-water being 1·0281, that of the Bottom-water was only 1·0283. On grouping all the observations on the Density of bottom-water, as indicated alike by Specific Gravity and by the proportion of Chlorine, into three series, according to Depth, the following curious result was arrived at:—

Fathoms.				Chlorine.	Sp. gr.
200 to 400,	Mean of	7	observations	21·53	1·0287
400 „ 800	„	7	„	21·38	1·0285
1300 „ 1700	„	6	„	21·21	1·0283

Thus it appears that the excess of Salinity is greatest in the *shallower* water, and that it gradually diminishes with the depth. This is also shown most strikingly by comparing the sample taken from the *least* depth (207 fathoms) with that taken from the greatest depth (1703 fathoms); for it was the former that showed the *maximum* of 21·88, and the latter that showed the *minimum* of 21·08. Now this fact seems not difficult to account for, if we consider the mode in which the concentration of the surface-film will be likely to affect the water below. For it can be shown experimentally, by pouring a strong saline solution tinged with colour upon the top of a weaker colourless solution, that the former will in the first instance sink "bodily," but will gradually impart its excess of salt to the liquid through which it falls; the descent of the coloured stratum becoming slower and slower, and its colour being more and more imparted to the general mass of the liquid. The proportion of salt will in time be made uniform throughout the whole column by "diffusion." Now it is obvious that if each column rests (so to speak) on its own base, the degree in which the Salinity of the whole mass is raised by the addition of a more concentrated solution will depend *cæteris paribus* upon its height; and thus where the depth of the Mediterranean basin is only between 200 and 400 fathoms, we might expect the Specific Gravity of its water to be more raised by the successive concentration of its surface-films, than where the depth ranges from 1300 to 1700 fathoms, —as proves to be actually the case.

Temperature.—Nothing can be more marked than the contrast between the *Temperature*-phenomena of the Mediterranean and those of the Atlantic. The Temperature-soundings taken on the coast of Spain and Portugal confirm in a remarkable manner the deductions drawn in former Discourses, as to the southward underflow of Arctic water along the floor of the Atlantic basin; as will be seen on comparing the two following Tables.

Making allowance for difference of Latitude, and excluding the influence of direct Solar radiation in *super*heating the *surface*-layer, the correspondence between these temperatures down to 350 fathoms is remarkably close. But whilst the same slow rate of reduction con-

tinues in No. II. down to 800 fathoms,—the bottom-temperature at 802 fathoms being 49°·3,—the reduction is more rapid in No. I., so that the temperature of 45°·5 is reached at 600 fathoms, whilst at 800 fathoms the temperature falls to 42°. Below 800 fathoms, however, in No. II., the temperature undergoes so extraordinarily rapid a depression, that it is reduced *nine degrees* within the next 200 fathoms; the water at 994 fathoms being only 2° warmer than it is at 1000 fathoms in No. I. A slight further reduction of temperature is noticeable in the two deepest soundings taken in the Atlantic Cruise of 1870, the temperature found at nearly 1100 fathoms (No. II.) being just 2° higher than that found at 1250 in No. I.

TEMPERATURE OF THE SEA AT DIFFERENT DEPTHS NEAR THE MARGIN OF THE NORTH ATLANTIC BASIN.

I.			II.		
Chops of the Channel, 1869. Average Latitude, 49°.			Coast of Spain and Portugal, 1870. Average Latitude, 39°.		
Depth, in Fathoms.	Surface-Temp. ° Fahr.	Bottom-Temp. ° Fahr.	Depth, in Fathoms.	Surface-Temp. ° Fahr.	Bottom-Temp. ° Fahr.
75	66·0	49·7	81	60·5	53·5
96	63·4	51·3	128	61·5	52·5
250	62·6	50·2	248	64·7	51·7
300	62·6	49·6	332	60·5	51·5
350	62·6	49·1	340	67·0	50·5
450	62·6	47·6	469	69·7	51·5
557	63·0	47·0			
600	62·6	45·5	620	67·3	50·5
725	63·9	43·9	718	66·5	50·5
			722	67·5	49·7
750	62·6	42·5	740	66·7	49·0
800	62·6	42·0	802	66·5	49·3
862	62·6	39·7	994	69·5	40·3
1000	64·0	38·3	1065	65·0	39·7
1250	64·0	37·7	1095	68·0	39·7

Considering these facts in the light thrown upon the Temperature phenomena of the Atlantic Basin by those of the "Cold Area" explored in 1869, it appears clear that we have in the Latitude of Lisbon the same distinct separation between an *upper warm* and a *lower cold* stratum as presented itself in the channel between the Shetland and the Faroe Islands; but whilst the "stratum of intermixture" in the latter lies between 150 and 300 fathoms, it lies in the former between 800 and 1000 fathoms. It seems perfectly clear that the *lower* stratum must have had a Polar source; but there is no evidence that the *upper* stratum is derived from any source nearer the Equator. Its temperature, indeed, is *lower* by 4° or 5° than that of the Mediterranean in the same parallel of Latitude at corresponding depths; and since the tem-

perature of the latter may be considered as the *normal* of the Latitude, —this great Inland Sea being virtually excluded from participation in the general Oceanic Circulation,—it would seem that the effect of that circulation is rather to lower than to raise the temperature of the upper stratum of this portion of the Atlantic. Its Surface-temperature during the summer is decidedly *lower* than that of the Mediterranean under the same parallel; and the limitation of the *super*heating to its most superficial layer is in entire accordance with our Mediterranean observations upon this point. As far as can be gathered from the data we at present possess, the *winter* Surface-temperature of this portion of the Atlantic is scarcely, if at all, higher than that of the Mediterranean under the same parallels. Hence it seems a justifiable conclusion that neither the superficial layer, nor any portion of the upper stratum, of the Atlantic water that laves the coasts of Spain and Portugal receives any accession of heat from the extension of the Gulf-stream into its area.

The daily averages of the Surface-temperature of the Mediterranean between the 16th of August and the 28th of September, ranged between 73° and 79°. This great elevation, however, is confined to a comparatively shallow stratum, as is shown in the following Table of Serial Soundings taken at three different Stations:—

	I.		II.		III.	
	° Fahr.	Diff.	° Fahr.	Diff.	° Fahr.	Diff.
Surface	74·5		69·5		77·0	
		5·2		10·5		6·0
5 fathoms					76·0	
10 „	69·3		59·0		71·0	
		4·3		1·5		9·5
20 „	65·0		57·5		61·5	
		2·0		1·0		1·5
30 „	63·0		56·5		60·0	
		1·3		0·8		2·7
40 „	61·7		55·7		57·3	
		2·0		0·4		0·6
50 „	59·7		55·3		56·7	
		4·6		0·6		1·2
100 „	55·1		54·7		55·5	

The *first* of these Stations, although the nearest to the Strait of Gibraltar, seems to have been out of the direct influence of its cold incurrent, which shows itself very strongly in the *second*, alike in the lower Surface-temperature, and in the reduction of 10°·5 in the first ten fathoms. The *third* may perhaps be taken as representing most characteristically the thermal condition of the upper stratum of the water of the Mediterranean during the season of greatest heat; and we here observe that while the Thermometer fell only 1° in the *first five* fathoms, it fell 5° in the *second five*, and no less than 9°·5 between 10 and 20 fathoms, below which depth the reduction was very slow. At 100 fathoms' depth, there was not a difference of more than 0°·8 between the temperatures at the three Stations.

The uniformity which showed itself in the Bottom-temperature *at all depths* below 100 fathoms was very remarkable.* The *lowest* Bottom-

* This uniformity had been previously noticed by M. Aimé and Captain Spratt; but as their observations were made with unprotected thermometers, their accuracy could not be relied on.

temperature anywhere met with was 54°; and this presented itself at a depth of 790 fathoms. The *highest* anywhere met with was 56°·5; and this presented itself in three instances, at depths of 266, 390, and 445 fathoms. But that the trifling elevation was not in any way dependent upon the smaller depth of these soundings, was obvious from two considerations:—first, that the deepest sounding gave a temperature of 56° on a bottom of 1743 fathoms, whilst 55° was found at depths of 1456 and 1508 fathoms; and second, that the slight variations observable among the *Bottom*-temperatures occurred also between the temperatures taken at 100 fathoms. In fact, *whatever the temperature was at* 100 *fathoms, that was the temperature of the whole mass of water beneath, down to the greatest depth explored.* In that part of the Western basin of the Mediterranean which lies between Gibraltar and Sardinia, the Bottom-temperature ranged between 54° and 55°·5, the *average* being 54°·9. East of this, in the neighbourhood of Sicily, the Bottom-temperature ranged between 55° and 56°·5, the *average* being 55°·8. It was because it was thought possible that the slight excess of Bottom-temperature on this area might be due to volcanic heat beneath, that our homeward course was directed by way of Etna and Stromboli, for the purpose of ascertaining if the near neighbourhood of a constantly active volcano has any influence in raising the temperature of the bottom. No such influence, however, was perceptible, the temperatures there obtained—*viz.* 55°·7 at 392 fathoms, and 55°·3 at 730 fathoms—being rather *below* than above the average.

The remarkable contrast thus presented to the slow but continuous reduction of temperature encountered in the successive strata of Oceanic water in the great Atlantic basin, and to the sudden fall which presents itself as the thermometer descends to its lower depths, excites inquiry into the cause of the difference. It is now clear that no amount of *surface*-heat has power *directly* to affect the temperature of sea-water to a greater depth than 100 fathoms, the elevation of temperature it produces below 30 fathoms being very slight; and it seems also clear that the uniform temperature of from 54° to 56°·5 encountered below the 100 fathoms' stratum, represents the *permanent temperature* of the great mass of water which occupies the Mediterranean basin. Now this mass is entirely cut off from the influence of the General Oceanic Circulation; the surface-inflow through the Strait of Gibraltar having no other effect than slightly to lower the general temperature at the western extremity of the basin. And the uniform permanent temperature of the mass of Mediterranean water may thus be considered as representing the mean temperature of the earth in that region, slightly raised perhaps, by a *downward convection* of heat from the surface in the manner to be presently described. With such an allowance, it corresponds closely with the determinations of the mean temperature of the crust of the earth in Europe, made by sinking thermometers into the ground to such a depth as to seclude them from the direct influence of summer heat or winter cold, but not to bring them within the direct influence of the internal heat of the earth.

The temperature of deep caves gives another set of data of the like kind, which accord very closely with the foregoing. Thus, Mr. Pengelly states that the temperature in the part of Kent's Hole, at Torquay, which is farthest from its entrance, varies but little from 52° throughout the year. There is a cave in the island of Pantellaria, lying between Sicily and the African coast, which is reputed to be of "icy coldness;" but Lieut. Millard, of H.M.S. 'Newport,' who has lately been making a careful survey of the island, informed us that although he felt it "very cold" on passing into it out of a very hot sunshine, its actual temperature, taken by thermometer, was 54°. And we have also learned, on good authority, that this is the temperature of the bottom of the deepest tanks in which water is stored up in Malta, provided that these are excavated (as is very commonly the case) beneath the houses, or are in any other way secluded from the direct rays of the sun.

The superficial stratum of the water of the Mediterranean being cooled down during the winter months to the uniform temperature of the depths below, we have to inquire in what manner it would be affected by the heating power of the summer sun. This, it is obvious, can be only exerted *directly* upon the actual surface; for the *conducting* power of water is so small, that very little downward transmission of heat would take place through its agency. Further, as the application of heat at the surface will render the superficial layer specifically lighter, no such *convection* will take place in the *downward* direction as takes place *upwards* when heat is applied at the bottom. But another agency comes into play in the case of sea-water. The rapid evaporation produced by powerful solar radiation, especially when aided by the hot dry winds of Africa, occasions such a concentration of the surface-film, that, in spite of its elevation of temperature, it becomes specifically heavier, and descends—to be replaced by a fresh layer. In this manner it will carry down an excess of heat, which diffuses itself through the subjacent layer, of course producing the greatest elevation of temperature in the stratum nearest the surface. The continual repetition of this process through the hot season will carry the elevation of temperature farther and farther down; but so soon as the temperature of the air falls much *below* that of the sea, the surface-layer being cooled will become heavier and sink, and will thus carry down *cold* instead of heat, so as to lower the temperature of the stratum below.

Solid Matter in Suspension.—The water of the Mediterranean is distinguished from that of the Atlantic, not only in the larger proportion of saline matter which it holds in *solution*, but also in having diffused through its whole mass, in a state of *suspension*, particles of solid matter in an extremely fine state of division. The Bottom-water taken up from great depths, was nearly always found to be turbid; and this turbidity was produced by particles of such extreme fineness, that it was with difficulty removed by filtration. Now it is a fact well known to Chemists and Physicists, that the length of time required for the

deposit of a precipitate increases with the fineness of the division of its particles, notwithstanding that the material of which they are composed may be of very high Specific Gravity. Thus it was shown by Faraday that precipitates of Gold may not subside for a month; and Mr. Babbage has calculated that, in the case of lighter substances, a period of hundreds of years may be required for the gravitation of very finely divided particles through a considerable mass of fluid. Now as the degree of this turbidity was found to bear a general relation to the depth,—being greatest where the superincumbent column was the highest,—it may be fairly concluded that the perceptible turbidity of the Bottom-water is due to the imperceptible diffusion of the same finely divided matter throughout the entire mass of superincumbent water. And that this is really the case, is shown by two different methods of proof. We learned from the Engineer of the Peninsular and Oriental Company's steam-ship by which we proceeded to join the 'Porcupine' at Gibraltar, that the deposit removed from the boilers after working in the Mediterranean differs from that left by Atlantic water, not only in its larger proportion of *salt*, but in having a very finely divided *mud* diffused through it, which is, of course, derived from the evaporation of *Surface*-water. The result of this large-scale experiment harmonizes exactly with Prof. Tyndall's examination of a small sample of the Surface-water of the Mediterranean by the Electric light; for he found it to be charged with very minute particles in suspension, as is also the water of the Lake of Geneva. And he has further shown that it is in each case to the presence of these particles, that we are to attribute the peculiar intensity of the blue colour by which both these waters are characterized.*

If we inquire into the source of these suspended particles, the progressive subsidence of which gives rise to the fine muddy deposit that covers all the deeper parts of the Mediterranean, we find that (so far, at least, as the Western basin is concerned) they have been in all probability brought down into it by the Rhone. The upper part of that river, as is well known, is constantly transporting a vast mass of sedimentary matter into the Lake of Geneva; and while the deposit of the coarser particles of the sediment at the upper end of the Lake is causing a progressive formation of alluvial land, the water which passes off at the lower end, though apparently clear, is still charged with particles in a finer state of division. The Western basin of the Mediterranean stands in the same relation to the lower part of the Rhone and to the tributaries which discharge themselves into it, that the Lake of Geneva does to its upper part. And a like universal diffusion of fine sedimentary particles through the Eastern basin is probably effected by the transporting agency of the Nile.

The very slow, but constant, subsidence of these minute sedimentary particles, then, is the source of a large part of the material of that fine tenacious mud which, mingled with a larger or smaller proportion

* See 'Nature,' Oct. 18, 1870.

of sand, partly calcareous and partly silicious, constitutes the deposit at present in progress on the deeper parts of the Mediterranean sea-bed. The source of the calcareous sand, which is itself in a state of very minute subdivision, is probably to be found in the abrasion of the Calcareous Tertiaries which form the shore-line round a large part of the Western basin. This abrasion is specially noticeable at Malta, where, for the security of the fortifications, it has been found necessary to check it by artificial means. The singular barrenness of this deposit in regard to animal life forced itself upon our attention during the whole of our Dredging-operations in the Mediterranean;* and though disappointed as Zoologists in not meeting with the novelties we hoped to encounter, the negative result of our sedulous investigations seems to have an important Geological bearing.

Our previous researches have fully demonstrated the fact, that the depth of from 600 to 1200 fathoms is not *per se* inconsistent with the existence of a varied and abundant Fauna; and that the reduction which shows itself at from 1200 to 2435 fathoms seems to depend as much on depression of temperature, as on increase of depth. Hence it was fairly to be expected that a varied and abundant Fauna—probably containing a number of Tertiary types supposed to have been long extinct—would have been found between 500 and 1500 fathoms, on a bottom of which the temperature seems never to fall below 54°. Now the question as to the cause of the deficiency of Animal life on this bottom naturally connects itself with the old geological difficulty, of which the inquiries of Prof. E. Forbes were long supposed to afford a satisfactory solution, *viz.* the existence of vast thicknesses of sedimentary strata, almost or entirely destitute of Organic remains. The explanation which has been accepted for many years,—that these deposits were formed in seas too deep to allow of the existence of animals on their bottom,—having been now shown to be untenable, the old difficulty recurs; and it is obvious that if it can be shown that a condition prejudicial to animal life now prevails on the Mediterranean bottom, which also prevailed when other azoic deposits were formed, a great step will have been gained. Such a condition is to be found in the *turbidity of the Bottom-water*. All marine animals are dependent for the aeration of their fluids on the contact of water either with their external surface, or with special (branchial) prolongations of it. Now if this water be charged with suspended particles of extreme fineness, the deposit of these particles upon the respiratory surface will interfere with the aerating process, and will tend to produce asphyxia. This is not a mere hypothesis. It is well known that oyster-beds cannot be established in situations to which fine mud is

* The question has been asked, why the shallower bottom nearer shore does not show the like barrenness. The answer is simply that the sedimentary mud is not deposited there in anything like the same quantity. The amount of such sediment deposited in a given time will depend upon the amount held in suspension in the column above; and this *cæteris paribus* will be proportional to its height.

brought by any fluvial or tidal current. And Mr. Jeffreys, when dredging some years ago in the neighbourhood of Spezzia, having on one occasion passed a little out of the bay, from a sandy bottom rich in animal life to a muddy bottom (this mud being doubtless a part of the Rhone deposit), without any considerable increase of depth, was forcibly struck by the barrenness of the latter.

It will be for Geologists to say how far this explanation can be applied to the case of the azoic sedimentary deposits of former epochs. One very notable case of the kind has been observed by Dr. Duncan; that of the Fleisch, a stratum not less than 6000 feet thick, extending from Mont Blanc to the Styrian Alps, which must have been deposited in the condition of extremely fine arenaceous mud, and in which there is an almost entire absence of fossils. The extremely fine calcareous sandstone of Malta, though reputed to be rich in fossils, seems to contain these fossils, for the most part, in its coarser beds, which were probably deposited in shallower waters, like those which we found rich in animal life along the shores of the Mediterranean. The extremely fine stone that is used for carved work,—so entirely wanting in "grain" that carvings executed in it look like casts in plaster of Paris—contains, we were assured, few fossils except Sharks' teeth, which, of course, dropped into it from above.

There is another condition, however, which may be not less potent in restraining within very narrow limits the Animal life of the deeper parts of the Mediterranean basin, namely, *the stagnation produced by the almost entire absence of Vertical Circulation.* In the great Oceanic basins, if the doctrine advanced in previous Discourses be correct, every drop of water is in its turn brought to the surface, and exposed to the purifying influence of prolonged exposure to the air. From this movement, the water of the Mediterranean may be said to be virtually excluded; and the deeper part of the basin has no circulation of its own, either horizontal or vertical, which will have the effect of bringing its water to the surface. It is difficult, in fact, to conceive of *any* agency that can disturb the stillness of the abyssal depths of a basin, which is completely shut in by a wall that rises more than 10,000 feet from its bottom. How far this affects the condition of such depths, in respect to the diffusion of the Organic matter and of the Oxygen required for the support of animal life, must be a matter of future inquiry.

Gibraltar Current.—A current almost invariably sets *eastwards* through the Strait of Gibraltar (or from the Atlantic into the Mediterranean), usually running at the rate of from two to three miles an hour. This current does not occupy by any means the entire breadth of the Strait, even at its narrowest part; its average width being there not more than four miles. On either side there is a stream, which when moving inwards is much less rapid, while its direction is periodically reversed under the influence of the lunar tide; so that at given times there are two lateral currents, the united force and amount of which, however, never approach those of the central inflow. The explanation of the

Gibraltar Current that has been most generally accepted, is that originally given by Dr. Halley, who attributed it to the excess of evaporation from the surface of the Mediterranean over the whole amount of water returned to it, either directly by rainfall, or by the rivers which discharge themselves into it; so that its level would be progressively lowered, if not kept up by the Atlantic inflow. The obvious objection to this explanation is, that as the water which passes off by evaporation leaves its salt behind it, and as the water which comes from the Atlantic brings its salt with it, there would be a progressive increase in the Salinity of the water of the Mediterranean, which does not appear to be the case. This objection has been met by another hypothesis, *viz.* that although the *surface*-water of the Mediterranean does not show more than a slight excess of saltness, there may be a great increase in the proportion of the quantity of salt held in solution in the waters of its abyssal depths; and it has even been surmised that a deposit of salt may be taking place on its bottom. This hypothesis must be considered as having been now finally disproved by the observations already summarized. From these observations it seems a justifiable inference that whilst, as maintained by Dr. Halley, the surface-evaporation from the Mediterranean exceeds the amount of fresh-water returned into it, the increase of Density which would result from the continual inflow of salt-water to maintain the level, is in some way kept in check, probably through an efflux of the denser water by an under-current, as originally suggested by Dr. Smith, one of the earliest inquirers into the matter, in 1673. And though the existence of the "ridge" between Capes Trafalgar and Spartel has been supposed to furnish a disproof of the existence of any such under-current, the force of this objection disappears when it is borne in mind that the average depth of water upon this "ridge" equals that of the British Channel, and that there are passages across it of double that depth.

The inquiries made expressly to determine this question in the 'Porcupine' Expedition of last summer were of a twofold character. It was our object in the *first* place to detect, if possible, by *mechanical* means, any movement which may be taking place in the deeper water, in opposition to the superficial inflow; and *secondly*, to determine by the Temperature, the Specific Gravity, and the Chemical composition of samples of water taken up at different points and from different depths, whether they belonged to the Mediterranean or to the Atlantic basin.

The *mechanical* method was entirely devised and carried out by Staff-Captain Calver, with the practical ability for which he is distinguished. The *physical* and *chemical* observations, which were made under the speaker's own direction, gave results which harmonized completely with those of the mechanical, where both could be employed together; and supply a deficiency which the impossibility of applying the mechanical test on the uneven surface of the comparatively shallow ridge, would otherwise have left in the proof of the outflow of Mediterranean water over it.

In the mid-stream between Gibraltar and Ceuta, at nearly the narrowest part of the Strait, where its depth exceeds 500 fathoms, it was conclusively proved by the action of Captain Calver's "current-drag" upon the boat from which it was suspended:—

(*a.*) That the current at 100 fathoms' depth flows *in*wards at less than half the rate of the surface-current.

(*b.*) That at 250 fathoms the direction of movement is completely reversed; the existence of an *outward* current being demonstrated by the dragging of the boat in direct opposition to the strong surface-current.

(*c.*) That at 400 fathoms there is still an *out*ward movement though at a reduced rate.

Again, it was found by examining samples of water taken from the surface, from 100 fathoms, from 250 fathoms, and from 400 fathoms, respectively, that, whilst the first two had the characteristic temperature and density of Atlantic water, the last two had the characteristic temperature and density of Mediterranean water; and it was specially remarkable that, whilst they both presented an excess alike in Specific Gravity and in the proportion of their Saline constituents, which clearly indicated their derivation from the Mediterranean basin, the samples taken from 250 fathoms' depth, on two different occasions, were much denser than that taken from 400 fathoms. It seems obvious that this heavier stratum could not be kept so near the surface, above a stratum of inferior density, by any other means than a *current* of no inconsiderable force.

Similar observations were afterwards carried on at the Western extremity of the Strait, where its breadth is greatly increased, and its depth proportionably diminished. The soundings taken along the line of a transverse section between Capes Trafalgar and Spartel show a great inequality of the bottom; channels of from 154 to 190 fathoms' depth existing in the immediate neighbourhood of shallows less than 50 fathoms from the surface. As was to be expected from the greater breadth of this part of the Strait, the *in*current flows at a much lower speed than at its Eastern extremity; its rate being reduced from nearly 3 miles to little more than 1¼ mile per hour. The use of the "current-drag" at 100 fathoms' depth, in a part of the channel of which the depth was 147 fathoms, did not indicate any reduction in this rate; but a decided retardation showed itself when the current-drag was lowered to 150 fathoms in a part of the channel of which the depth approached 200 fathoms. As it was deemed inexpedient to lower the "current-drag" to a greater depth, since it would have been almost certain to "foul" on the rocky bottom, it could not be ascertained by Mechanical means that the stratum of water *immediately* lying over the ridge has the outward movement which it might be expected to show. But by means of the Water-bottle and Thermometers it was ascertained that the waters of the deeper channels intersecting the "ridge" were no less distinctly *Mediterranean* than that of the under-

current near Gibraltar; and since it is obvious that this could not have continued to maintain itself against the inflow of Atlantic water, unless it had an outward movement of its own, the existence of an under-current over the deeper portion of the ridge cannot be reasonably doubted.

It is clear, therefore, that the Mediterranean water which has undergone concentration by evaporation, is being continually carried back into the Atlantic by this westerly under-current, which causes it to *move up a gradual incline* from the eastern to the western extremity of the Strait; so that the excess of density in the Mediterranean water is thus kept within a very narrow limit.

The *rationale* of this double current offered by Captain Maury—on the assumption, now proved to be correct, of the existence of an under-current—was thought by the speaker to account satisfactorily for its phenomena. As the surface-level of the Mediterranean is lowered by the excess of evaporation, it can only be maintained by an *in*flow. But, on the other hand, the maintenance of the *level* whilst the *density* of the Mediterranean water is in excess, disturbs the equilibrium between the columns of water at the two extremities of the Strait, the heavier water of the Mediterranean overbalancing the lighter water of the Atlantic; and thus a portion of the former is forced *out*wards as an under-current, thus again producing a depression of the level, to be restored by a surface inflow from the Atlantic. Now, since the Atlantic water which enters the Mediterranean is in its turn subjected to loss by evaporation and consequent increase of density, the excess on the Mediterranean side is constantly kept up; whilst, on the other hand, in consequence of the vastness of the mass of water in the Atlantic basin, and the dilution it receives elsewhere, the discharge of the heavier water of the Mediterranean produces no perceptible increase in *its* Specific Gravity. And thus, as the lowering of the level and the disturbance of the equilibrium are being constantly reproduced, the superficial *in*flow and the deep *out*flow will be as constantly maintained by the force of Gravity.

It seems a fair test of the validity of this explanation, that it is found applicable *mutatis mutandis* to the converse case of the double current in the Baltic Sound. The Baltic receives the drainage of one-fifth of Europe, and its evaporation is comparatively small; so that its level would be progressively *raised*, if the excess did not pass off by its three outlets—the Sound, the Great Belt, and the Little Belt—into the North Sea. Now, just as the water of the Mediterranean would gradually *increase* in saltness, if its density were not kept down by exchange with the Atlantic, so would that of the Baltic progressively *diminish* in saltness, by the washing-out of its shallow basin (so to speak) with river-water, if there were no return-current beneath, bringing back denser water from the North Sea. But since we find, as a matter of fact, that its density is pretty uniformly the same, being about one-sixth that of Ocean-water, it is obvious that there *must* be an under-current bringing back as much salt as is carried out

by the surface-current. The force of this inferential proof could scarcely be increased by experimental verification. Such verification, however, was obtained two hundred years ago (as recorded by Dr. Smith, who first advanced the hypothesis of the Gibraltar under-current) by a mechanical method resembling that employed by the 'Porcupine' in the Strait of Gibraltar; and a further verification has recently been afforded by the inquiries of Prof. Forchhammer, who has obtained the comparatively dense water of the Kattegat from the bottom of the Sound.

The maintenance of this under-current in the Baltic Sound will be a necessary consequence of the constant inequality between the weights of columns of water of equal heights in the Baltic and the North Sea; not, as in the case of the Atlantic and the Mediterranean, through the increase of the Specific Gravity of the water in one column by continual concentration, but through the reduction in the Specific Gravity of the other by continual dilution. The excessive inflow of fresh water will continually tend to *raise* the level of the Baltic; and this will produce an outward surface-current into the North Sea. But since the *density* of the Baltic column has been *lowered* by admixture with fresh water, and a portion of its salt has been carried off by the surface-outflow, the restoration of the level will make the Baltic column the *lighter;* and the difference of downward pressure in the two columns will cause an inflow of the lower portion of the North Sea column, until the equilibrium is restored. But this will, in its turn, tend to renew the elevation in the level of the Baltic column, which will be further raised by the continued influx of fresh water; and thus a renewed surface-*out*flow will take place to restore the level, and a renewed deep *in*flow to restore the equilibrium.

A little consideration will make it obvious that a like Vertical Circulation must take place in *any* case in which a want of level and a want of equilibrium between two columns of water are constantly maintained, whatever may be the agency concerned in producing them; and thus a constant difference in Temperature will have exactly the same effect as alteration of bulk and density by evaporation or dilution.

Suppose two basins of Ocean-water, connected by a deep Strait, to be placed under such different climatic conditions, that the surface of one is exposed to the heating influence of Tropical sunshine, whilst the surface of the other is subjected to the extreme cold of the sunless Polar winter. The effect of the surface-*heat* upon the water of the Tropical basin, being for the most part limited to its *upper stratum*, may be practically disregarded. But the effect of surface-*cold* upon the water of the Polar basin will be to reduce the temperature of its *whole mass* below the freezing-point of fresh water; the surface-stratum sinking, as it is cooled, in virtue of its diminished bulk and increased density, and being replaced by water not yet cooled to the same degree. Thus the *level* of the Polar column will be sensibly lowered, whilst its *weight* undergoes no reduction; and whilst an *in*flux of

surface-water will take place from the Tropical basin to take the place of that which has gone down, the greater weight which will thus be imparted to the Polar column will cause an *efflux* of its cold deep water for the restoration of the balance. So long, then, as the warm water which passes into the Polar basin from the Tropical is subjected to the cooling influence of its atmosphere, and is in its turn sent down to the bottom by the increase in its density, so long will its continual descent keep up an influx from the Tropical basin; and so long as that influx is maintained, a corresponding efflux will be produced from the bottom of the Polar basin into the bottom of the Equatorial. As the surface-water of the latter is constantly drawn off into the former, the water which has entered it from below is gradually lifted up by what follows it, and thus at last comes to the surface, where it is in its turn exposed to the heating influence of the Tropical sun, and is thence drawn off into the Polar basin, to repeat the same circulation.

The case is not essentially altered by the existence of free communication between the two basins, through a large body of intermediate water; the principal difference in the result being that the movement in each direction will be slower, whilst it will affect a larger mass. The action of Cold on the surface-water of each Polar area will be exerted as follows:—

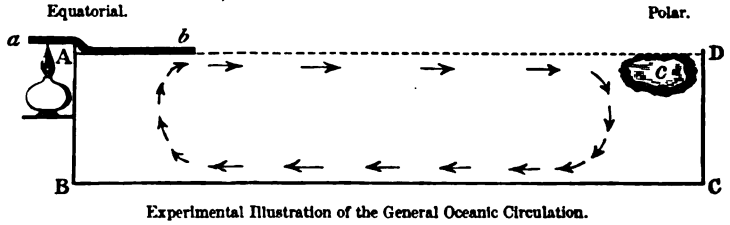
(*a.*) In diminishing the height of the Polar column as compared with that of the Equatorial, so that a lowering of its *level* is produced, which can only be made good by a surface-flow from the latter towards the former.

(*b.*) In producing an excess in the downward *pressure* of the column, when this inflow has restored its level, in virtue of the increase of Specific Gravity it has gained by its reduction of volume; whereby a portion of its heavy bottom-water is displaced laterally, causing a further reduction of level that draws in a further supply of the warmer and lighter water flowing towards its surface.

(*c.*) In imparting a downward *movement* to each new surface-stratum as its temperature undergoes reduction; so that the entire column may be said to be in a state of constant descent, like that which exists in the water of a tall jar when an opening is made at its bottom, and the water which flows away through it is replaced by an equivalent supply poured into the top of the jar.

The ice-cold water thus pressed away from the Polar areas will travel along the floor of the deep Ocean-basins continuous with their own, beneath the warmer water which it encounters there, and with which it will undergo a certain intermixture at their plane of junction; so that, as every fresh arrival will take its place beneath that which has preceded it, there will be a gradual *upward* movement of Polar water as it approaches the Equator, until it comes under the direct heating influence of the Tropical sun, to be again drawn towards the Poles by the tractive agency already described.

The following experiment affords an illustration of this doctrine:—



Experimental Illustration of the General Oceanic Circulation.

A B C D is a long narrow trough, with glass sides, filled with water nearly to its edge, as indicated by the dotted line A D. At A a metallic bar *a b*, is so fixed that one part of it lies along the surface of the water, whilst the other, which projects beyond the trough, is heated by a spirit-lamp placed beneath. At D a piece of ice, *c*, is wedged in between the two sides of the trough. Thus the water in the trough is acted on by *surface-heat* at one end, like the water of the Equatorial ocean, and by *surface-cold* at the other, like the water of the Polar Sea. By the introduction of *blue* colouring liquid at the surface near D, and of *red* colouring liquid near *b*, it is shown that a continuous circulation is kept up in the direction of the arrows: the *blue* liquid, as it is cooled by the ice, at once descends to C, then travels slowly along the bottom, from C towards B, gradually rises towards the heated bar, and thence creeps along the surface back towards D; whilst the *red* liquid first travels along the surface from A to D (to take the place of that which has descended), then itself descends to C on being cooled by the ice, returns along the bottom from C to B, and, lastly, mounts again towards A.

Here, then, we have a *vera causa* for a *General Oceanic Circulation*, which, being sustained simply by the unequal distribution of Solar Heat, will be entirely independent of any peculiar distribution of land and water that does not prevent the free communication between the Polar and Equatorial areas, in their depths as well as at their surface. That this agency has been hitherto so little recognized by Physical Geographers can only be attributed to the prevalence of the erroneous idea of a uniform deep-water temperature of 39°; of which idea the observations made in the 'Lightning' Expedition of 1868, confirmed and extended by those of the 'Porcupine' Expedition of 1869, have completely proved the fallacy. Until it is clearly apprehended that Sea-water becomes more and more dense as its temperature is reduced, and that it consequently continues to sink until it freezes, the immense motor power of Polar Cold cannot be apprehended. But when this has been clearly recognized, it is seen that the application of *cold at the surface* is precisely equivalent as a moving power to that application of *heat at the bottom*, by which the Circulation of water is sustained in every Heating Apparatus that makes use of it.

That such a Vertical Circulation really takes place in Oceanic water, and that its influence in moderating the excessive Cold of the

Polar areas and the excessive Heat of the Equatorial region, is far more important than that of any surface-currents, seems a legitimate deduction from the facts stated in the previous Discourse. For, on the one hand, it was shown that there is a general diffusion of an almost glacial temperature on the bottom of the deep Ocean-basins, which, at depths exceeding 1000 fathoms, are occupied by Polar water,* more or less diluted by admixture according to the length of the course it has had to travel; whilst between this stratum and that other stratum of warmer water which (on the hypothesis) is slowly moving Polewards, there is a "stratum of intermixture," in which there is such a rapid change of Temperature as might be expected from the relation of the upper and lower masses of water. This "stratum of intermixture" showed itself in a most marked manner in the Atlantic Temperature-observations of the present Expedition (p. 5, No. II.); the descent of the Thermometer, which had been very slow with increase of depth between 100 and 800 fathoms, becoming suddenly augmented in rate; so that between 800 and 1000 fathoms it fell *nine degrees*, namely, from 49°·3 to 40°·3.

On the other hand, it was shown in the previous Discourse that there is evidence of the slow Pole-ward movement of a great upper stratum of Oceanic water, carrying with it a warm temperature; which movement cannot be attributed to any such local influences as those which produce the Gulf-stream or any other currents put in motion by *surface*-action. Of such a movement, it was contended, we have a marked example in that north-easterly flow which conveys the warmth of Southern latitudes to the West of Ireland and Scotland, the Orkney, Shetland, and Faroe Islands, Iceland, Spitzbergen, and the Polar basin generally. This flow, of whose existence conclusive evidence is derived from observations of the Temperature of these regions, is commonly regarded as a prolongation of the Gulf-stream; and this view is maintained not only by Dr. Petermann, who has recently collected and digested these observations with the greatest care, but also by Prof. Wyville Thomson, as well as by Mr. Croll. Having elsewhere fully stated his objections to this doctrine, and discussed the validity of the arguments adduced in support of it,† the speaker would here only record the conclusions which a careful examination of the present state of our knowledge of the subject has led him to form:—

I. There is no evidence, either from the Surface-temperature of the Sea, or from the temperature of sea-board Stations along the western coast of Southern Europe, that the Climate of that region is ameliorated by a flow of Ocean-water having a temperature higher than that of the Latitude;—the Surface-temperature of the Mediterranean Sea, which is virtually excluded from all Oceanic Circulation,

* A temperature of 33½° has lately been observed by Commander Chimmo (who was furnished with the "protected" Miller-Casella Thermometers) at a depth of 2306 fathoms, nearly under the Equator.

† 'Proceedings of the Royal Geographical Society' for Jan. 9, 1871.

being higher than that of the eastern margin of the Atlantic in corresponding latitudes, and the Climate of sea-board Stations on the Mediterranean being warmer than that of Stations corresponding to them in Latitude on the Atlantic Coast; and this not merely in summer, but also in winter. This Oceanic region may therefore be designated the *neutral area*.

II. The evidence of Climatic amelioration increases in proportion as we pass Northwards from the *neutral area*, becoming very decided at the Orkney, Shetland, and Faroe Islands; but that, as was shown by the 'Porcupine' Temperature-soundings of 1869, the flow of warm water which produces this amelioration extends to a depth of at least 700 fathoms, though the temperature of its lower 200 fathoms is reduced by the influence of the Polar underflow (p. 5, No. I.).

III. This deep stratum of Warm water appears, from the correspondence in the rate of its diminution of Temperature with increase of depth, to be derived from the *neutral area* to the south-west; as is shown in the following comparison of the Temperatures down to 500 fathoms, obtained (in 1869) by a serial Sounding at a point about 150 miles to the north-west of Scotland, and the Bottom-temperatures obtained off Lisbon in 1870. It will be seen that whilst the *surface*-temperatures differ, in accordance with the respective temperatures of the Air, by 15°·4, the difference between the temperatures observed from 100 to 500 fathoms varies only between 4°·9 and 5°·7.

N. Lat.	Temperature of Air.	Temperature of Sea.					
		Surface.	Fahr. 100.	Fahr. 200.	Fahr. 300.	Fahr. 400.	Fahr. 500.
° ′	°	°	°	°	°	°	°
59 35	54	52·6	47·3	46·8	46·6	46·1	45·1
About 39	70	68·0	53·0	52·0	51·5	51·0	50·5

A comparison of these data brings into prominence the superior heating power of a deep mass of warm water, above that of a mere surface-layer. For we see that whilst the temperature of the upper stratum is rapidly cooled down in its northward passage, that of the 400 fathoms beneath it sustains comparatively little reduction. The thinned-out margin of the Tropical Gulf-stream would lose all its heat long before it reaches the Polar Circle; but the deep flow which carries northwards the warmth of a Temperate area yields it up far more gradually.

IV. This slow Pole-ward movement of the upper warm layer of the North Atlantic, down to the "stratum of intermixture," is exactly what might be expected to take place as the *complement* of the flow of glacial water from the Polar to the Equatorial area; the two movements constituting a General *vertical* Oceanic Circulation.

V. There is a strong probability that the quantity of *Water* discharged by the Gulf-stream has been greatly over-estimated, in con-

sequence of the rate of the surface-current having been assumed as the rate of movement through the whole sectional area, which is contrary to all analogy; whilst there is also a strong probability that there is a *reverse* under-current of *cold* water through the Narrows, derived from the Polar current that is distinctly traceable nearly to its mouth. The upper stratum of this southerly current comes to the surface between the Gulf-stream and the coast of the United States; whilst its deeper and colder stratum underlies the Gulf-stream itself. That there is a slow southerly movement of Arctic water beneath the Gulf-stream, is indicated by the fact that icebergs have been seen moving southwards in direct opposition to its surface-flow, their deeply immersed portion presenting a larger surface to the lower stratum than their upper part does to the more superficial layer,—as in the case of our "current-drag." And similar evidence is afforded by the southward drift of the buoy which was attached to the Atlantic Cable of 1865, but which broke away from it, apparently carrying with it a great length of the wire rope by which it had been attached.

VI. There is a strong probability that the quantity of *Heat* carried off by the water of the Gulf-stream has been greatly over-estimated; the Temperature-soundings taken during the cruise of the 'Porcupine' in the Mediterranean having shown that the very high temperature of the surface extends but a little way down, whilst the Temperature-observations in the Atlantic show that the descent into a cold stratum beneath may be very rapid. Hence the average of 65° assumed by Mr. Croll on the basis of observations made at considerable intervals of depth, is altogether unreliable.

VII. The most recent and trustworthy observations indicate that the edge of the Gulf-stream to the north-east of the Banks of Newfoundland, is so thinned out and broken up by interdigitation with Polar currents, that its existence *as a continuous current* beyond that region cannot be proved by observations, either of Temperature or Movement. The last distinctly traced edge has a thickness certainly *not exceeding* 50 fathoms, and not improbably less.

VIII. The Gulf-stream and other local currents put in motion by the Trade-winds or other influences acting on the *surface* only, will have as *their* complement in a *horizontal* circulation return *surface*-currents; and the horizontal circulation of which the Atlantic Equatorial Current and the Gulf-stream constitute the first part, is completed—so far as the Northern Hemisphere is concerned—partly by the direct return of one large section of the Gulf-stream into the Equatorial Current, and, as to the other section, by the *superficial* Polar currents, which make their way southwards, the principal of them even reaching the commencement of the Gulf-stream.

In conclusion, it may be added that the doctrine of a General *vertical* Oceanic Circulation is in remarkable accordance with the fact now placed beyond doubt by the concurrent evidence of a great number of observations, that whilst the Density of Oceanic water, which

is lowest in the Polar area, progressively increases as we approach the Tropics, it again shows a decided reduction in the Intertropical area. It has been thought that an explanation of this fact is to be found in the large amount of rainfall, and of inflow of fresh water from great rivers, in the Intertropical region; but it is to be remembered that the surface-evaporation also is there the most excessive; so that some more satisfactory account of the fact seems requisite. Such an explanation is afforded by the doctrine here advocated; the more dilute Polar water which flows along the bottom of the Ocean-basins towards the Equator being there (so to speak) pumped up and brought to the surface. And it is not a little confirmatory of the views advanced in this Discourse, that in a recent elaborate discussion of the facts relating to the comparative Density of Oceanic Water on different parts of the Earth's surface, the doctrine of a General *vertical* Circulation is advocated as affording the only feasible rationale of them.*

[The speaker wishes to state that he claims no originality in regard to the doctrine of a General Oceanic Circulation sustained by difference of Temperature. It has been advanced by various writers, especially Prof. Buff and Capt. Maury; though, so far as he is aware, it has not been fully developed by any. If he has himself done anything to strengthen the doctrine, it has been by showing that Polar Cold, rather than Equatorial Heat, is the *primum mobile* of this circulation; and further, by bringing a large number of phenomena, apparently unrelated, under the comprehension of the same Physical Theory.]

Addendum.—The limitation of time having prevented the speaker from giving more than a slight notice of the Zoological results of the Expedition, he is desirous of introducing as an Appendix some extracts from the Report presented to the Royal Society, chiefly relating to the First Cruise off the coast of Portugal under charge of Mr. J. Gwyn Jeffreys.

Wednesday, July 20th.—Dredged all day with considerable success at depths of from 380 to 994 fathoms (lat. 40° N., long. 9° 50′ W.). The dredgings in 380 and 469 fathoms yielded among the Mollusca *Leda lucida* (Norwegian, and a Sicilian fossil), *Axinus cumyarius* (also Norwegian), *Neæra obesa* (Spitzbergen to the west of Ireland), *Odostomia* sp. n. *O. minuta* (Mediterranean), and *Cerithium* (sp. n.); and among the Echinoderms were *Brisinga endecacnemos* and *Asteronyx Loveni* (Norwegian). But the results of the Dredging in 994 fathoms were so extraordinary as to excite our utmost astonishment. It being late in the evening, the contents of the dredge could not be sifted and examined until daylight the next morning. We then saw a marvellous assemblage of Shells, mostly dead, and consisting of Pteropods, but comprising certain species which we had always regarded as exclusively

* "Densité Salure et Courants de l'Océan Atlantique," par Lieut. B. Savy, Annales Hydrographiques,' 1868, p. 620.

Northern, and others which Mr. Jeffreys recognized as Sicilian Tertiary fossils; while nearly forty per cent. of the entire number of species were undescribed, and some of them represented new genera. The following is an analysis of the Mollusca (perfect and fragmentary) taken in this one haul :—

Orders.	Total Number of Species.	Recent.	Fossil.	New or Undescribed.
Brachiopoda	1	1		
Conchifera	50	32	1	17
Solenoconchia	7	3	..	4
Gastropoda	113	42	23	48
Heteropoda	1	1		
Pteropoda	14	12	..	2
	186	91	24	71

The Northern species above referred to are 34 in number, and include *Mytilus* (*Dacrydium*) *vitreus*, *Nucula pumila*, *Leda lucida*, *L. frigida*, *Pecchiolia abyssicola*, *Neæra jugosa* or *lamellosa*, *N. obesa*, *Tectura fulva*, *Fissurisepta papillosa*, *Cyclostrema* (sp. n.), *Torellia vestita*, *Pleurotoma turricula*, *Admete veridula*, *Cylichna alba*, *Cylichna ovata*, Jeffr. MS. = *Bulla conulus*, S. Wood, not Deshayes (Coralline Crag), and *Scaphander librarius*. *Leda lucida*, *Neæra jugosa*, *Tectura fulva*, *Fissurisepta papillosa*, *Torellia vestita*, and the undescribed species of *Cylichna*, as well as several other known species in this dredging, are also fossil in Sicily. Nearly all these Shells, as well as a few small Echinoderms, Corals, and other organisms, had evidently been transported by some current to the spot where they were found; and they must have formed a thick deposit, similar to those of which many Tertiary fossiliferous strata are composed. None of the shells were Miocene, or of an older period. This remarkable collection, of which not much more than one-half is known to Conchologists, notwithstanding their assiduous labours, teaches us how much remains to be done before we can assume that the record of Marine Zoology is complete. Besides the Mollusca in this dredging from 994 fathoms, Professor Duncan informs us that there are two new genera of Corals, and *Flabellum distinctum*, which last he regards as identical with one from North Japan. It coincides with the discovery on the Lusitanian coasts of two Japanese species of a curious genus of Mollusca (*Pecchiolia* or *Verticordia*), both of which are fossil in Sicily, and one of them in the Coralline Crag of Suffolk. Professor Wyville Thomson notices undescribed Sponges from the same dredging.

Thursday, July 21st.—Dredged all day in from 600 to 1095 fathoms (lat. 39° 40′ N., long. 9° 40′ W.) with extraordinary success. Together with many of the new and peculiar species of Mollusca obtained at the last station, some of which were here alive, occurred—*Nucula delphinodonta*, *Leda* (*Yoldia*) sp. n., *L. abyssicola*, *Axinus*

eumyarius, *Siphonodentalium vitreum* (the first being North American and Norwegian, and the last three Arctic and Norwegian), *S. coarctatum* (a well-known Subapennine fossil), *Dischides* (sp. n.), *Chiton albus* (northern), *Molleria costulata* (Arctic), *Trochus reticulatus* (a Calabrian fossil), *Omphalius monocingulatus*, Seg. MS. (a Sicilian fossil), *Hela* (sp. n.), *Eulima* (sp. n.), *Scalaria frondosa* (a Leghorn and Crag fossil), and *Trachysma delicatum* (a Sicilian fossil). Of Crustacea, there were *Apseudes spinosa* (Norwegian and British), *A. grossimanus* (sp. n.), and *Paranthura elongata* (sp. n.). Of Polyzoa, *Cellepora abyssicola*, sp. n. (Busk MS.). Of Corals, *Cœnocyathus* (sp. n.), and an undescribed species of an unknown genus allied to *Bathycyathus*. *Holtenia Carpenteri* and other rare Sponges, with *Brisinga endecacnemos* and various Echinoderms equally interesting, formed part of our treasures; but the greatest prize of all was a noble *Pentacrinus*, about a foot long, of which several specimens came up attached to the tangles. This discovery of a true *Pentacrinus* in the European seas crowned the day's work: Mr. Jeffreys has named it *P. Wyville-Thomsoni*. Our *Pentacrinus* was dragged up from soft mud or ooze, and its base was entirely free. Portions of the arms occurred in several other dredgings on the Lusitanian coasts; and joints of apparently the same species have been found by Professor Seguenza in the Zanclean formation or older Pliocene near Messina.

July 27th.—Dredged a few miles north of Cape St. Vincent, in 292 and 374 fathoms. The last haul yielded two Silicious or Vitreous Sponges of an enormous size, one of them measuring nearly 3 feet in diameter at the top, of the kind called "Neptune's Cup" (*Askonema Setubalense*, Kent), besides that lovely sponge, *Aphrocallistes Bocagei*. The Mollusca were mainly northern, and some were Sicilian fossils. Two undescribed species of Crustacea, which Mr. Norman proposes to name *Amathia Jeffreysi* and *Ethusa mirabilis*, were here obtained.

July 29th.—Steamed south, and dredged in 364 and 322 fathoms, between Cape St. Vincent and Cadiz. The Mollusca comprised some of the new and remarkable species procured in the 994 fathoms and other dredgings, as well as *Terebratula vitrea*, *T. cranium*, *Pholadomya* (sp. n.), *Trochus amabilis*, *Pyramidella plicosa* (Belgian and Coralline Crag), *Tylodina Dubeni* (Norwegian), *Cancellaria mitræformis*, *C. subangulosa* (both Coralline Crag), *Pleurotoma galerita*, and *Actæon pusillus*. The most remarkable novelty here obtained was a large collection of thin sandy disks, from 0·3 to 0·4 inch in diameter, with a slight central prominence; for these proved on subsequent examination to contain an entirely new type of *Actinozoon*, extraordinarily flattened in form, and entirely destitute of tentacles. Dr. Carpenter, by whom this curious organism will be described, has assigned to it the name of *Ammodiscus Lindahli*.

Aug. 3rd.—Dredged in 477, 651, and 554 fathoms, across the entrance to the Straits of Gibraltar, and towards the Morocco coast; and we shifted our ground at night. The Fauna was northern, but scanty; the bottom being stiff clay, and nearly unproductive. Un-

described species of *Cioniscus* and *Bulla* were among the Mollusca; a remarkable Sponge, 18 inches long, which Prof. Wyville Thomson considers the type of a new genus allied to *Esperia* (*Chondrocladia virgata*, Wyv. Th. MS.), another new and exquisitely graceful Sponge of the *Holtenia* group, and provisionally named by him *Pheronema* (?) *velatum*, and *Aphrocallistes Bocagei;* two new species of a compound stony Coral (*Cœnocyathus*); and a few Crustaceans and Annelids were taken.

On Tuesday, Sept. 6, we resumed our Dredging-explorations on the shallow between the Eastern and Western basins of the Mediterranean, that extends between the African coast and Sicily, and is termed the "Adventure Bank." The depths here range from about 30 to 250 fathoms. We here found, between 25 and 85 fathoms, the following species of Mollusca: *Trochus saturalis*, Ph. (Sic. foss.); *Xenophora crispa*, König. (Sic. foss.); *Cylichna striatula*, Forb. (Sic. foss.); *C. ovulata*, Brocchi (Sic. foss.). And seven miles off the point called Rinaldo's Chair, between 60 and 160 fathoms, we obtained *Tellina compressa*, Brocchi (Sic. foss.), a species possessing the following synonyms—*Tellina striatula*, Calcara; *T. strigilata*, Ph.; *Psammobia Weinkauffi*, Crosse; and *Angulus Macandrei*, Sowerby: also an interesting Annelid, *Praxilla prætermissa*, Malmgren (northern). Here, again, we brought up a great abundance of Polyzoa; and many of these have proved of great interest. One, in particular, of a beautiful, very open reticular plan of growth, is the type of a new genus, of which another species had been previously obtained by Mr. Busk from the Canary Islands, and which he will describe under the name of *Climacopora*. Many of the species obtained had been previously known only as Tertiary Fossils. Abundance of Shells were here found; among them we obtained a considerable number of living specimens of *Megerlia truncata*, including a whole series in various stages of growth, the youngest of which presented a very remarkable character,—a set of *setæ* projecting from the margin of the shell, the length of which exceeded its own long diameter. Among other species of interest were:—*Kellia*, sp. n. (Sic. foss.); *Gadinia excentrica*, Tiberi; *Rissoa* (sp. n.); *Scalaria frondosa*, J. Sow. (Sic. & Cor. Cr. foss.); *Odostomia unifasciata*, Forbes; *Pyramidella plicosa*, Bronn (Sic. & Cor. Cr. foss.) = *P. læviuscula*, S. Wood; and *Actæon pusillus*, Forb. (Sic. foss.). Among the Annelida was an interesting Northern form, *Hyalinœcia tubicola*, Müller. The Echinodermata were in considerable abundance, but were mostly well-known Mediterranean types. The *Cidaris hystrix* was especially frequent; and a comparison of the series of specimens obtained in this and the preceding Cruise, with those obtained last year in the Northern area, has enabled Prof. Wyville Thomson to satisfy himself that *C. hystrix*, *C. papillata*, and *C. affinis* are specifically identical. The Corals found on the Adventure Bank have proved peculiarly interesting, as belonging to the *Rugose* division previously supposed to be entirely extinct.

[W. B. C.]

WEEKLY EVENING MEETING,

March 17, 1871.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

J. Norman Lockyer, Esq.

On the Eclipse.

[Abstract deferred.]

WEEKLY EVENING MEETING,

Friday, March 24, 1871.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Professor J. Clerk Maxwell, M.A. LL.D. F.R.S.

On Colour Vision.

All vision is colour vision, for it is only by observing differences of colour that we distinguish the forms of objects. I include differences of brightness or shade among differences of colour.

It was in the Royal Institution, about the beginning of this century, that Thomas Young made the first distinct announcement of that doctrine of the vision of colours which I propose to illustrate. We may state it thus:—We are capable of feeling three different colour-sensations. Light of different kinds excites these sensations in different proportions, and it is by the different combinations of these three primary sensations that all the varieties of visible colour are produced. In this statement there is one word on which we must fix our attention. That word is, Sensation. It seems almost a truism to say that colour is a sensation; and yet Young, by honestly recognizing this elementary truth, established the first consistent theory of colour. So far as I know, Thomas Young was the first who, starting from the well-known fact that there are three primary colours, sought for the explanation of this fact, not in the nature of light, but in the constitution of man. Even of those who have written on colour since the time of Young, some have supposed that they ought to study the

properties of pigments, and others that they ought to analyze the rays of light. They have sought for a knowledge of colour by examining something in external nature—something out of themselves.

Now, if the sensation which we call colour has any laws, it must be something in our own nature which determines the form of these laws; and I need not tell you that the only evidence we can obtain respecting ourselves is derived from consciousness.

The science of colour must therefore be regarded as essentially a mental science. It differs from the greater part of what is called mental science in the large use which it makes of the physical sciences, and in particular of optics and anatomy. But it gives evidence that it is a mental science by the numerous illustrations which it furnishes of various operations of the mind.

In this place we always feel on firmer ground when we are dealing with physical science. I shall therefore begin by showing how we apply the discoveries of Newton to the manipulation of light, so as to give you an opportunity of feeling for yourselves the different sensations of colour.

Before the time of Newton, white light was supposed to be of all known things the purest. When light appears coloured, it was supposed to have become contaminated by coming into contact with gross bodies. We may still think white light the emblem of purity, though Newton has taught us that its purity does not consist in simplicity.

We now form the prismatic spectrum on the screen [*exhibited*]. These are the simple colours of which white light is always made up. We can distinguish a great many hues in passing from the one end to the other; but it is when we employ powerful spectroscopes, or avail ourselves of the labours of those who have mapped out the spectrum, that we become aware of the immense multitude of different kinds of light, every one of which has been the object of special study. Every increase of the power of our instruments increases in the same proportion the number of lines visible in the spectrum.

All light, as Newton proved, is composed of these rays taken in different proportions. Objects which we call coloured when illuminated by white light, make a selection of these rays, and our eyes receive from them only a part of the light which falls on them. But if they receive only the pure rays of a single colour of the spectrum they can appear only of that colour. If I place this disk, containing alternate quadrants of red and green paper, in the red rays, it appears all red, but the red quadrants brightest. If I place it in the green rays both papers appear green, but the red paper is now the darkest. This, then, is the optical explanation of the colours of bodies when illuminated with white light. They separate the white light into its component parts, absorbing some and scattering others.

Here are two transparent solutions [*exhibited*]. One appears yellow, it contains bichromate of potash; the other appears blue, it contains sulphate of copper. If I transmit the light of the electric lamp through the two solutions at once, the spot on the screen appears green. By

means of the spectrum we shall be able to explain this. The yellow solution cuts off the blue end of the spectrum, leaving only the red, orange, yellow, and green. The blue solution cuts off the red end, leaving only the green, blue, and violet. The only light which can get through both is the green light, as you see. In the same way most blue and yellow paints, when mixed, appear green. The light which falls on the mixture is so beaten about between the yellow particles and the blue that the only light which survives is the green. But yellow and blue light when mixed do not make green, as you will see if we allow them to fall on the same part of the screen together.

It is a striking illustration of our mental processes that many persons have not only gone on believing, on the evidence of the mixture of pigments, that blue and yellow make green, but that they have even persuaded themselves that they could detect the separate sensations of blueness and of yellowness in the sensation of green.

We have availed ourselves hitherto of the analysis of light by coloured substances. We must now return, still under the guidance of Newton, to the prismatic spectrum. Newton not only

"Untwisted all the shining robe of day,"

but showed how to put it together again. We have here a pure spectrum, but instead of catching it on a screen we allow it to pass through a lens large enough to receive all the coloured rays. These rays proceed, according to well-known principles in optics, to form an image of the prism on a screen placed at the proper distance. This image is formed by rays of all colours, and you see the result is white. But if I stop any of the coloured rays the image is no longer white, but coloured; and if I only let through rays of one colour, the image of the prism appears of that colour.

I have here an arrangement of slits by which I can select one, two, or three portions of the light of the spectrum, and allow them to form an image of the prism while all the rest are stopped. This gives me a perfect command of the colours of the spectrum, and I can produce on the screen every possible shade of colour by adjusting the breadth and the position of the slits through which the light passes. I can also, by interposing a lens in the passage of the light, show you a magnified image of the slits, by which you will see the different kinds of light which compose the mixture.

The colours are at present red, green, and blue, and the mixture of the three colours is, as you see, nearly white. Let us try the effect of mixing two of these colours. Red and blue form a fine purple or crimson, green and blue form a sea-green or sky-blue, red and green form a yellow.

Here again we have a fact not universally known. No painter, wishing to produce a fine yellow, mixes his red with his green. The result would be a very dirty drab colour. He is furnished by nature with brilliant yellow pigments, and he takes advantage of these. When he mixes red and green paint, the red light scattered by the red paint is

robbed of nearly all its brightness by getting among particles of green, and the green light fares no better, for it is sure to fall in with particles of red paint. But when the pencil with which we paint is composed of the rays of light, the effect of two coats of colour is very different. The red and the green form a yellow of great splendour, which may be shown to be as intense as the purest yellow of the spectrum.

I have now arranged the slits to transmit the yellow of the spectrum. You see it is similar in colour to the yellow formed by mixing red and green. It differs from the mixture, however, in being strictly homogeneous in a physical point of view. The prism, as you see, does not divide it into two portions as it did the mixture. Let us now combine this yellow with the blue of the spectrum. The result is certainly not green; we may make it pink if our yellow is of a warm hue, but if we choose a greenish yellow we can produce a good white.

You have now seen the most remarkable of the combinations of colours—the others differ from them in degree, not in kind. I must now ask you to think no more of the physical arrangements by which you were enabled to see these colours, and to concentrate your attention upon the colours you saw, that is to say on certain sensations of which you were conscious. We are here surrounded by difficulties of a kind which we do not meet with in purely physical inquiries. We can all feel these sensations, but none of us can describe them. They are not only private property, but they are incommunicable. We have names for the external objects which excite our sensations, but not for the sensations themselves.

When we look at a broad field of uniform colour, whether it is really simple or compound, we find that the sensation of colour appears to our consciousness as one and indivisible. We cannot directly recognize the elementary sensations of which it is composed, as we can distinguish the component notes of a musical chord. A colour, therefore, must be regarded as a single thing, the quality of which is capable of variation.

To bring a quality within the grasp of exact science, we must conceive it as depending on the values of one or more variable quantities, and the first step in our scientific progress is to determine the number of these variables which are necessary and sufficient to determine the quality of a colour. We do not require any elaborate experiments to prove that the quality of colour can vary in three and only in three independent ways.

One way of expressing this is by saying, with the painters, that colour may vary in hue, tint, and shade.

The finest example of a series of colours, varying in hue, is the spectrum itself. A difference in hue may be illustrated by the difference between adjoining colours in the spectrum. The series of hues in the spectrum is not complete; for, in order to get purple hues, we must blend the red and the blue.

Tint may be defined as the degree of purity of a colour. Thus, bright yellow, buff, and cream-colour, form a series of colours of nearly the same hue, but varying in tint. The tints, corresponding to any given hue, form a series, beginning with the most pronounced colour, and ending with a perfectly neutral tint.

Shade may be defined as the greater or less defect of illumination. If we begin with any tint of any hue, we can form a gradation from that colour to black, and this gradation is a series of shades of that colour. Thus we may say that brown is a dark shade of orange.

The quality of a colour may vary in three different and independent ways. We cannot conceive of any others. In fact, if we adjust one colour to another, so as to agree in hue, in tint, and in shade, the two colours are absolutely indistinguishable. There are therefore three, and only three, ways in which a colour can vary.

I have purposely avoided introducing at this stage of our inquiry anything which may be called a scientific experiment, in order to show that we may determine the number of quantities upon which the variation of colour depends by means of our ordinary experience alone.

Here is a point in this room: if I wish to specify its position, I may do so by giving the measurements of three distances—namely, the height above the floor, the distance from the wall behind me, and the distance from the wall at my left hand.

This is only one of many ways of stating the position of a point, but it is one of the most convenient. Now, colour also depends on three things. If we call these the intensities of the three primary colour sensations, and if we are able in any way to measure these three intensities, we may consider the colour as specified by these three measurements. Hence the specification of a colour agrees with the specification of a point in the room in depending on three measurements.

Let us go a step farther and suppose the colour sensations measured on some scale of intensity, and a point found for which the three distances, or co-ordinates, contain the same number of feet as the sensations contain degrees of intensity. Then we may say, by a useful geometrical convention, that the colour is represented, to our mathematical imagination, by the point so found in the room; and if there are several colours, represented by several points, the chromatic relations of the colours will be represented by the geometrical relations of the points. This method of expressing the relations of colours is a great help to the imagination. You will find these relations of colours stated in an exceedingly clear manner in Mr. Benson's 'Manual of Colour,' one of the very few books on colour in which the statements are founded on legitimate experiments.

There is a still more convenient method of representing the relations of colours by means of Young's triangle of colours. It is impossible to represent on a plane piece of paper every conceivable colour, to do this requires space of three dimensions. If, however, we consider only colours of the same shade—that is, colours in which

the sum of the intensities of the three sensations is the same, then the variations in tint and in hue of all such colours may be represented by points on a plane. For this purpose we must draw a plane cutting off equal lengths from the three lines representing the primary sensations. The part of this plane within the space in which we have been distributing our colours will be an equilateral triangle. The three primary colours will be at the three angles, white or gray will be in the middle, the tint or degree of purity of any colour will be expressed by its distance from the middle point, and its hue will depend on the angular position of the line which joins it with the middle point.

Thus the ideas of tint and hue can be expressed geometrically on Young's triangle. To understand what is meant by shade we have only to suppose the illumination of the whole triangle increased or diminished, so that by means of this adjustment of illumination Young's triangle may be made to exhibit every variety of colour. If we now take any two colours in the triangle and mix them in any proportions, we shall find the resultant colour in the line joining the component colours at the point corresponding to their centre of gravity.

I have said nothing about the nature of the three primary sensations, or what particular colours they most resemble. In order to lay down on paper the relations between actual colours, it is not necessary to know what the primary colours are. We may take any three colours, provisionally, as the angles of a triangle, and determine the position of any other observed colour with respect to these, so as to form a kind of chart of colours.

Of all colours which we see, those excited by the different rays of the prismatic spectrum have the greatest scientific importance. All light consists either of some one kind of these rays, or of some combination of them. The colours of all natural bodies are compounded of the colours of the spectrum. If therefore we can form a chromatic chart of the spectrum, expressing the relations between the colours of its different portions, then the colours of all natural bodies will be found within a certain boundary on the chart defined by the positions of the colours of the spectrum.

But the chart of the spectrum will also help us to the knowledge of the nature of the three primary sensations. Since every sensation is essentially a positive thing, every compound colour-sensation must be within the triangle of which the primary colours are the angles. In particular, the chart of the spectrum must be entirely within Young's triangle of colours, so that if any colour in the spectrum is identical with one of the colour-sensations, the chart of the spectrum must be in the form of a line having a sharp angle at the point corresponding to this colour.

I have already shown you how we can make a mixture of any three of the colours of the spectrum, and vary the colour of the mixture by altering the intensity of any of the three components. If we place this compound colour side by side with any other colour, we can alter

the compound colour till it appears exactly similar to the other. This can be done with the greatest exactness when the resultant colour is nearly white. I have therefore constructed an instrument which I may call a colour-box, for the purpose of making matches between two colours. It can only be used by one observer at a time, and it requires daylight, so I have not brought it with me to-night. It is nothing but the realization of the construction of one of Newton's propositions in his 'Lectiones Opticæ,' where he shows how to take a beam of light, to separate it into its components, to deal with these components as we please by means of slits, and afterwards to unite them into a beam again. The observer looks into the box through a small slit. He sees a round field of light consisting of two semicircles divided by a vertical diameter. The semicircle on the left consists of light which has been enfeebled by two reflexions at the surface of glass. That on the right is a mixture of colours of the spectrum, the positions and intensities of which are regulated by a system of slits.

The observer forms a judgment respecting the colours of the two semicircles. Suppose he finds the one on the right hand redder than the other, he says so, and the operator, by means of screws outside the box, alters the breadth of one of the slits, so as to make the mixture less red; and so on, till the right semicircle is made exactly of the same appearance as the left, and the line of separation becomes almost invisible.

When the operator and the observer have worked together for some time, they get to understand each other, and the colours are adjusted much more rapidly than at first.

When the match is pronounced perfect, the positions of the slits, as indicated by a scale, are registered, and the breadth of each slit is carefully measured by means of a gauge. The registered result of an observation is called a "colour equation." It asserts that a mixture of three colours is, in the opinion of the observer (whose name is given), identical with a neutral tint, which we shall call Standard White. Each colour is specified by the position of the slit on the scale, which indicates its position in the spectrum, and by the breadth of the slit, which is a measure of its intensity.

In order to make a survey of the spectrum we select three points for purposes of comparison, and we call these the three Standard Colours. The standard colours are selected on the same principles as those which guide the engineer in selecting stations for a survey. They must be conspicuous and invariable and not in the same straight line.

In the chart of the spectrum you may see the relations of the various colours of the spectrum to the three standard colours, and to each other. It is manifest that the standard green which I have chosen cannot be one of the true primary colours, for the other colours do not all lie within the triangle formed by joining them. But the chart of the spectrum may be described as consisting of two

straight lines meeting in a point. This point corresponds to a green about a fifth of the distance from *b* towards F. This green has a wave length of about 510 millionths of a millimètre by Ditscheiner's measurement. This green is either the true primary green, or at least it is the nearest approach to it which we can ever see. Proceeding from this green towards the red end of the spectrum, we find the different colours lying almost exactly in a straight line. This indicates that any colour is chromatically equivalent to a mixture of any two colours on opposite sides of it, and in the same straight line. The extreme red is considerably beyond the standard red, but it is in the same straight line, and therefore we might, if we had no other evidence, assume the extreme red as the true primary red. We shall see, however, that the true primary red is not exactly represented in colour by any part of the spectrum. It lies somewhat beyond the extreme red, but in the same straight line.

On the blue side of primary green the colour equations are seldom so accurate. The colours, however, lie in a line which is nearly straight. I have not been able to detect any measurable chromatic difference between the extreme indigo and the violet. The colours of this end of the spectrum are represented by a number of points very close to each other. We may suppose that the primary blue is a sensation differing little from that excited by the parts of the spectrum near G.

Now, the first thing which occurs to most people about this result is that the division of the spectrum is by no means a fair one. Between the red and the green we have a series of colours apparently very different from either, and having such marked characteristics that two of them, orange and yellow, have received separate names. The colours between the green and the blue, on the other hand, have an obvious resemblance to one or both of the extreme colours, and no distinct names for these colours have ever become popularly recognized.

I do not profess to reconcile this discrepancy between ordinary and scientific experience. It only shows that it is impossible by a mere act of introspection to make a true analysis of our sensations. Consciousness is our only authority; but consciousness must be methodically examined in order to obtain any trustworthy results.

I have here, through the kindness of Professor Huxley, a picture of the structure upon which the light falls at the back of the eye. There is a minute structure of bodies like rods and cones or pegs, and it is conceivable that the mode in which we become aware of the shapes of things is by a consciousness which differs according to the particular rods on the ends of which the light falls, just as the pattern on the web formed by a Jacquard loom depends on the mode in which the perforated cards act on the system of movable rods in that machine. In the eye we have on the one hand light falling on this wonderful structure, and on the other hand we have the sensation of sight. We cannot compare these two things; they belong to opposite

categories. The whole of Metaphysics lies like a great gulf between them. It is possible that discoveries in physiology may be made by tracing the course of the nervous disturbance

"Up the fine fibres to the sentient brain;"

but this would make us no wiser than we are about those colour-sensations which we can only know by feeling them ourselves. Still, though it is impossible to become acquainted with a sensation by the anatomical study of the organ with which it is connected, we may make use of the sensation as a means of investigating the anatomical structure.

A remarkable instance of this is the deduction of Helmholtz's theory of the structure of the retina from that of Young with respect to the sensation of colour. Young asserts that there are three elementary sensations of colour; Helmholtz asserts that there are three systems of nerves in the retina, each of which has for its function, when acted on by light or any other disturbing agent, to excite in us one of these three sensations.

No anatomist has hitherto been able to distinguish these three systems of nerves by microscopic observation. But it is admitted in physiology that the only way in which the sensation excited by a particular nerve can vary is by degrees of intensity. The intensity of the sensation may vary from the faintest impression up to an insupportable pain; but whatever be the exciting cause, the sensation will be the same when it reaches the same intensity. If this doctrine of the function of a nerve be admitted, it is legitimate to reason from the fact that colour may vary in three different ways, to the inference that these three modes of variation arise from the independent action of three different nerves or sets of nerves.

Some very remarkable observations on the sensation of colour have been made by M. Sigmund Exner in Professor Helmholtz's physiological laboratory at Heidelberg. While looking at an intense light of a brilliant colour, he exposed his eye to rapid alternations of light and darkness by waving his fingers before his eyes. Under these circumstances a peculiar minute structure made its appearance in the field of view, which many of us may have casually observed. M. Exner states that the character of this structure is different according to the colour of the light employed. When red light is used a veined structure is seen; when the light is green, the field appears covered with minute black dots, and when the light is blue, spots are seen, of a larger size than the dots in the green, and of a lighter colour.

Whether these appearances present themselves to all eyes, and whether they have for their physical cause any difference in the arrangement of the nerves of the three systems in Helmholtz's theory I cannot say, but I am sure that if these systems of nerves have a real existence, no method is more likely to demonstrate their existence than that which M. Exner has followed.

Colour Blindness.

The most valuable evidence which we possess with respect to colour vision is furnished to us by the colour-blind. A considerable number of persons in every large community are unable to distinguish between certain pairs of colours which to ordinary people appear in glaring contrast. Dr. Dalton, the founder of the atomic theory of chemistry, has given us an account of his own case.

The true nature of this peculiarity of vision was first pointed out by Sir John Herschel in a letter written to Dalton in 1832, but not known to the world till the publication of Dalton's Life by Dr. Henry. The defect consists in the absence of one of the three primary sensations of colour. Colour-blind vision depends on the variable intensities of two sensations instead of three. The best description of colour-blind vision is that given by Professor Pole in his account of his own case in the 'Phil. Trans.,' 1859.

In all cases which have been examined with sufficient care, the absent sensation appears to resemble that which we call red. The point P on the chart of the spectrum represents the relation of the absent sensation to the colours of the spectrum, deduced from observations with the colour box furnished by Professor Pole.

If it were possible to exhibit the colour corresponding to this point on the chart, it would be invisible, absolutely black, to Professor Pole. As it does not lie within the range of the colours of the spectrum we cannot exhibit it; and, in fact, colour-blind people can perceive the extreme end of the spectrum which we call red, though it appears to them much darker than to us, and does not excite in them the sensation which we call red. In the diagram of the intensities of the three sensations excited by different parts of the spectrum, the upper figure, marked P, is deduced from the observations of Professor Pole; while the lower one, marked K, is founded on observations by a very accurate observer of the normal type.

The only difference between the two diagrams is that in the upper one the red curve is absent. The forms of the other two curves are nearly the same for both observers. We have great reason therefore to conclude that the colour sensations which Professor Pole sees are what we call green and blue. This is the result of my calculations; but Professor Pole agrees with every other colour-blind person whom I know in denying that green is one of his sensations. The colour-blind are always making mistakes about green things and confounding them with red. The colours they have no doubts about are certainly blue and yellow, and they persist in saying that yellow, and not green, is the colour which they are able to see.

To explain this discrepancy we must remember that colour-blind persons learn the names of colours by the same method as ourselves. They are told that the sky is blue, that grass is green, that gold is yellow, and that soldiers' coats are red. They observe difference in the colours

of these objects, and they often suppose that they see the same colours as we do, only not so well. But if we look at the diagram we shall see that the brightest example of their second sensation in the spectrum is not in the green, but in the part which we call yellow, and which we teach them to call yellow. The figure of the spectrum below Professor Pole's curves is intended to represent to ordinary eyes what a colour-blind person would see in the spectrum. I hardly dare to draw your attention to it, for if you were to think that any painted picture would enable you to see with other people's vision I should certainly have lectured in vain.

On the Yellow Spot.

Experiments on colour indicate very considerable differences between the vision of different persons, all of whom are of the ordinary type. A colour, for instance, which one person on comparing it with white will pronounce pinkish, another person will pronounce greenish. This difference, however, does not arise from any diversity in the nature of the colour sensations in different persons. It is exactly of the same kind as would be observed if one of the persons wore yellow spectacles. In fact, most of us have near the middle of the retina a yellow spot through which the rays must pass before they reach the sensitive organ: this spot appears yellow because it absorbs the rays near the line F, which are of a greenish-blue colour. Some of us have this spot strongly developed. My own observations of the spectrum near the line F are of very little value on this account. I am indebted to Professor Stokes for the knowledge of a method by which anyone may see whether he has this yellow spot. It consists in looking at a white object through a solution of chloride of chromium, or at a screen on which light which has passed through this solution is thrown [*exhibited*]. This light is a mixture of red light with the light which is so strongly absorbed by the yellow spot. When it falls on the ordinary surface of the retina it is of a neutral tint, but when it falls on the yellow spot only the red light reaches the optic nerve, and we see a red spot floating like a rosy cloud over the illuminated field.

Very few persons are unable to detect the yellow spot in this way. The observer K, whose colour equations have been used in preparing the chart of the spectrum, is one of the very few who do not see everything as if through yellow spectacles. As for myself, the position of white light in the chart of the spectrum is on the yellow side of true white even when I use the outer parts of the retina; but as soon as I look direct at it, it becomes much yellower, as is shown by the point W C. It is a curious fact that we do not see this yellow spot on every occasion, and that we do not think white objects yellow. But if we wear spectacles of any colour for some time, or if we live in a room lighted by windows all of one colour, we soon come to recognize white paper as white. This shows that it is only when some alteration takes place in our sensations, that we are conscious of their quality.

There are several interesting facts about the colour sensation which I can only mention briefly. One is that the extreme parts of the retina are nearly insensible to red. If you hold a red flower and a blue flower in your hand as far back as you can see your hand, you will lose sight of the red flower, while you still see the blue one. Another is, that when the light is diminished red objects become darkened more in proportion than blue ones. The third is, that a kind of colour blindness in which blue is the absent sensation can be produced artificially by taking doses of Santonine. This kind of colour blindness is described by Dr. Edmund Rose, of Berlin. It is only temporary, and does not appear to be followed by any more serious consequences than headaches. I must ask your pardon for not having undergone a course of this medicine, even for the sake of becoming able to give you information at first hand about colour blindness.

[J. C. M.]

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, March 3, 1871.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

CAPTAIN NOBLE, F.R.S. &c.
LATE ROYAL ARTILLERY,

On the Tension of Fired Gunpowder.

THE first attempt made to explain the action of gunpowder was, I believe, that of M. De la Hire, who, in the History of the French Academy for 1702, ascribed the force of fired gunpowder to the behaviour of the air enclosed in and between the grains of powder. This air he considered to be highly heated by the combustion of the charge, and the consequent elasticity to be the moving force of the projectile.

Robins, who followed M. De la Hire as the next writer on the subject, and who may be considered to have laid the foundation of this, as of so many other departments of artillery science, points out how inadequate to the effect are the forces supposed to act by M. De la Hire. He himself instituted a carefully-planned and well-conducted series of experiments, in which he determined the quantity of permanent gas generated by the explosion of gunpowder; adduced experiments which he considered to prove that this quantity is the same whether the powder be exploded in the air or in vacuo; and finally determined the increase of elasticity due to the supposed temperature of the explosion. The conclusions at which Robins arrived were briefly as follow:—1. That the whole action of the powder on the projectile was due to the permanent gases generated by the explosion. 2. That at ordinary temperature and atmospheric pressure the permanent gases occupied about 240 times the volume of the unexploded powder. 3. That the heat of combustion increased this volume to about 1000 times that of the powder, and that hence the maximum force of gunpowder—somewhat less with small, somewhat greater with large charges—was about 1000 atmospheres, that is to say about 6½ tons on the square inch. But although Robins considered this pressure the maximum exerted by fired gunpowder, it is worthy of remark that he recognized the intensity of the local pressure which arises when the gases generated have space sufficient to acquire a con-

siderable velocity before meeting with an obstacle. In a common musket he placed a bullet 16 inches from the charge, and found that at the seat of the shot the barrel was bulged like a bladder to twice its original diameter, while two pieces were blown out of it.

The first regular experiments which had for their object the determination of the pressure of gunpowder fired in a close vessel or chamber were those of Count Rumford, made in 1793, and published in the Transactions of the Royal Society for 1797. The powder used was sporting, of very fine grain, and its composition (there being only 67 per cent. of saltpetre), differed notably from ordinary powder. The charges used, moreover, were very small, the maximum being only 18 grains. In one case, indeed, the vessel was filled; about 28 grains were necessary to fill the chamber; but by this experiment the vessel was destroyed. The objects Rumford had in view were—first, to ascertain the limit of the force exercised by the exploded powder when the gases are at their maximum density; secondly, to determine the relation between the density of the gases and the tension. The results of the first and most reliable series of Rumford's experiments give the maximum tension at about 29,000 atmospheres, or 191 tons on the square inch. But, great as this pressure is, Count Rumford considers it much below the truth. He made a second series, the results of which, to use his own words, "are still more various, extraordinary, and inexplicable." The tension of the gas in the first series of experiments was with 12 grains of powder about 2700 atmospheres; but in this second series the pressure with the same charge is repeatedly found to be above 9000 atmospheres. Count Rumford does not attempt to explain the enormous discrepancy between the two sets of experiments, unless a remark on the heat of the weather during the second set can be so considered; but, relying on this second series and on the experiment in which the vessel was destroyed by 28 grains, he arrived at the conclusion that 101,021 atmospheres, or 662 tons on the square inch, is the measure of the initial force of the elastic fluid generated by the combustion of gunpowder. Rumford met the objection that if the tension were anything like that he names, no gun would have a chance of standing, by assuming that the combustion of the powder is much slower than is ordinarily supposed, and, indeed, lasts all the time the shot is in the bore; and he further accounts for the enormous initial tension by ascribing it to the elasticity of the aqueous vapour or steam contained in the powder. Reasoning, on M. de Betancourt's experiments, that the elasticity of steam is doubled by every addition of temperature equal to 30° Fahrenheit, his only difficulty, and one which he leaves to his successors to explain, is why the steam liberated by the combustion of the powder does not exercise a much higher pressure than the 100,000 atmospheres he has assigned to it.

In 1843 Colonel Cavalli proposed to insert in the bore of a gun a series of small barrels, intended to throw a wrought-iron spherical ball. By ascertaining the velocities of these balls Colonel Cavalli

considered that he would be able to assign the corresponding pressures. Colonel Cavalli's plan was actually carried out, and from his experiments he deduced what ought to be the theoretical thickness of the metal at various points along the bore.

A very great improvement on Colonel Cavalli's method was introduced in 1854 by a Prussian Artillery Committee, under the direction of General, then Major, Neumann. . . . But their experiments do not possess a very high interest, as they were applied only to comparatively very small guns, and had for their chief object the comparison between elongated and non-elongated cartridges. The general result seems to have been that in the 6-pr. gun the maximum pressure was about 1100 atmospheres, while in the 12-pr. it was nearly 1300 atmospheres, . . . and in every charge with which they experimented two maxima of tension were distinctly perceptible. The Russian artillerist, General Mayevski, in a memoir founded on these experiments, confirms the results at which the Prussian Committee have arrived, and points out that from the experiments the maximum pressure must be attained before the bullet is any considerable distance from its initial position. General Neumann's method appears to have been repeated in Belgium about the year 1860 with a 70-pr. rifled gun. I have not seen a detailed report of these trials, but the maximum pressure with ordinary powder was stated to be about 3000 atmospheres.

In 1857–8–9 Major Rodman carried on for the United States a most interesting and extensive series of experiments on gunpowder. [These were fully described by the aid of diagrams and tables.] He appears to have been the first person who experimented on the effect of size of grain, and proposed prismatic powder. It is most unfortunate that experiments so well devised, and carried out with so much care, should be rendered in many cases almost valueless by the absence of important data, by the admission of manifestly erroneous observations, and, finally, by results passed over in silence which are not only frequently anomalous, but in some cases absolutely impossible.

Major Rodman, like Count Rumford, endeavoured to ascertain the maximum force which powder was capable of exerting when fired in its own volume. He fired various charges in enormously strong shells, through a small vent $\frac{1}{10}''$ in diameter, and he considered that in all cases the maximum pressure would be exerted before the shell burst. His results were very diverse, varying from 32 tons per square inch (4900 atmospheres) to 82 tons, or about 12,400 atmospheres, and, singularly enough, the highest pressure was given by the smallest charge: from the great discrepancies, as well as from other considerations, I do not think we can accept these determinations as entitled to much weight.

MM. Bunsen and Schischkoff's experiments were directed, in the first place, to determine the exact nature, both of the permanent gases and the solid products generated by the explosion of powder; secondly,

to determine the heat generated by the act of explosion; thirdly, to determine the maximum pressure which gunpowder fired in a close chamber would give rise to; and, finally, to determine the total quantity of work which a given weight of gunpowder is capable of producing. The apparatus adopted for obtaining the products of combustion was so arranged that the powder to be analyzed falls in a very finely divided stream into a heated bulb, in which, and in tubes connected with it, the resulting products are collected for examination. MM. Bunsen and Schischkoff, in drawing attention to their results and the extraordinary difference between their estimates and those given by so eminent an authority as Piobert, point out that many of the assumptions previously made must depend on very faulty premises; but their own experiments have not altogether escaped attack, and I think we are bound to receive some of their results with great reservation, until it can be demonstrated that the products of combustion are the same in the bore of a gun as when produced in the method followed in these experiments.

The results of their analysis are given in this table.* The permanent gases at a temperature of zero and pressure of 760 mm., occupied a volume 193 times greater than that occupied by the powder, and represented about $\frac{31}{100}$ the weight of the powder. The remainder was solid residue, and MM. Bunsen and Schischkoff conceive, that although a portion of these solid matters may undoubtedly be volatilized by the high temperature of the explosion, yet any pressure which may be exerted by such vapours is quite insignificant. This opinion appears to be founded on the fact that the solid residue arising from the explosion of gunpowder is not fused when exposed to the action of a jet of inflamed hydrogen.

Piobert and other authorities, on the other hand, consider that the

* Transformation experienced by gunpowder in burning, after Bunsen and Schischkoff.

1 grm. powder.

Nitre		0·7899
Sulphur		0·0984
Charcoal	C	0·0769
	H	0·0041
	O	0·0307

Gave when exploded

	grm.		grm.	cub. cnts.
Residue	0·6806	$KOSO^3$	0·4227	
		$KOCO^2$	0·1264	
		KOS^2O^2	0·0327	
		KS	0·0213	
		KC_y, S^2	0·0030	
		$KONO^5$	0·0372	
		C	0·0073	
		S	0·0014	
		$2NH^4O, 3CO^2$	0·0286	
Gases	0·3138	N	0·0998	79·40
		CO^2	0·2012	101·71
		CO	0·0094	7·49
		H	0·0002	2·34
		HS	0·0018	1·16
		O	0·0014	1·00
	0·9944			193·10

pressure exerted by the volatilized residue has far more influence on the pressure than the permanent gases.

The temperature of the fired gunpowder was determined by exploding a small charge of powder enclosed in a tube, which was itself immersed in a larger tube containing water. From the increment of temperature communicated to the water by the explosion, it was found that one part of fired powder would raise 620 parts of water by 1° Cent., and hence it was calculated that the temperature of gunpowder fired in a close chamber impervious to heat is 3340 Cent., or 5980 Fahr.

Assuming, first, that the products obtained in the two methods I have just described are identical, and, secondly, that no variations in the products arise from the combustion of large charges, this result would be very near the truth.

The pressure in a closed vessel is readily deducible from the above data, and MM. Bunsen and Schischkoff compute that the maximum tension which the gas can attain—to which it may approximate, but can never reach—is about 4374 atmospheres, or about 29 tons on the square inch. This pressure has been undoubtedly reached in the case of heavy guns, and considerably exceeded in the case of powder fired in closed vessels. MM. Bunsen and Schischkoff also compute, from their data, the theoretical work of a kilogramme of gunpowder at 67,400 kilogrammètres, that is 67,400 kilogrammes raised 1 mètre in height. The committee on explosives have, however, realized in the shot alone nearly 60,000 kilogrammètres per kilogramme of powder in a comparatively short gun; and it may therefore be conjectured that this estimate, like that of the maximum pressure, is considerably too low, although undoubtedly much nearer the truth than the extravagant estimates which have frequently been made.

In the year 1861–2 Sir W. Armstrong and myself made several experiments to determine the maximum pressure of powder in the bores of what were then considered very large guns—the 110 and the 70 pounders. Two methods were adopted, and although they, like nearly every experiment connected with gunpowder, gave results in some degree anomalous, yet the conclusion at which we arrived—namely, that the maximum pressure with the powder then used, in the bores of the guns I have mentioned was about 17 tons on the square inch—is probably not far removed from the truth.

[The description of these methods was illustrated by diagrams of apparatus.]

It is not possible in anything like a reasonable time to give an analysis of the voluminous investigations of Piobert on the question of gunpowder. Generally, however, his views seem to be that he ascribes much of the initial pressure of gunpowder to the effects of the vaporized solid products increasing enormously the tension due to the pemanent gases. He points out errors in some of Rumford's conclusions, but accepts as tolerably accurate the pressures given by Rumford's first series, which would, at maximum density, give a tension of about 29,000 atmospheres.

A committee, under the presidency of Colonel Younghusband, has been recently appointed to examine into our gunpowder, which has for some years enjoyed on the Continent the unenviable denomination of "brutal powder."

The researches of this committee have been solely devoted in the first place to a special object—the production of a powder suitable for the monster guns which are now required by the services. . . . The guns we have principally used have been three in number—a gun of 2·1 in. diameter, firing projectiles of 4¼ lbs., and charges of 9 oz.; an 8-in. gun, firing projectiles of 180 lbs., and charges of from 20 to 40 lbs.; and a 10-in. gun, firing projectiles of 400 lbs., and charges of from 60 to 70 lbs. of powder. The means we have used to determine the pressure have been likewise three—first, a Rodman gauge; secondly, a crusher gauge, designed to overcome certain faults in the Rodman gauge; thirdly, a chronoscope, designed for measuring very minute intervals of time. . . .

The chronoscope consists of a series of thin disks, each 36 inches in circumference, fixed at intervals on a horizontal shaft, and driven at a high speed by a heavy descending weight, which is, during the experiment, continually wound up by a handle, and with a little practice the instrument can be made to travel either quite uniformly or at a rate very slowly increasing or decreasing. The precise rate of the disks is ascertained by means of a stop-clock, which can be connected or disconnected with a revolving shaft at pleasure. The speed with which the circumferences of the disks travel is in this instrument generally about 1200 inches per second. An inch therefore represents the 1200th part of a second, and as by means of a vernier we are able to divide the inch into 1000 parts, the instrument is capable of recording less than the one-millionth part of a second. I may mention, by way of enabling you to realize the extreme minuteness of this portion of time, that the millionth part of a second is about the same fraction of a second that a second is of a fortnight.

When the primary of an induction coil is suddenly severed, a spark under proper management is given off from the secondary, and in the arrangement I am describing, the severance of the primary is caused by the shot in its passage through the bore, and the record of its passage is transferred to the disks in the following way. The peripheries of the revolving disks are covered with strips of white paper coated with lampblack, and are connected with one of the secondary wires of an induction coil. The other secondary wire, carefully insulated, is brought to a discharger opposite to the edge of a disk, and fitted so as to be just clear of it. The primary wires of the induction coils are so connected with the bore of a gun that the shot in passing a definite point shall sever the primary current, and thereby produce a spark from the secondary. A hollow plug is screwed into the gun, carrying at the end next the bore a cutter, which projects slightly into the bore. The cutter is held in this position by the primary wire, which passes in at one side of the plug, then through

a hole in the cutter, and out at the other side of the plug. When the shot passes the cutter it presses it level with the surface of the bore, thereby severing the primary and causing the induced spark to pass instantaneously from the discharger to the disk, making a minute perforation in the paper covering upon the opposite part of the disk, and at the same time burning away the lampblack, so that the position of the perforation is marked by a white spot. There is an induction coil for each disk, and each disk, discharger, and coil form, so to speak, an independent instrument for recording the instant when the projectile passes a certain point in the bore of the gun. Before using the instrument, we must be satisfied that the various independent instruments give corresponding results, and the best mode of doing this is to get a record upon each disk of the same event.

Thus it is obvious that if the whole of the primaries are cut simultaneously, the sparks on all the disks should be in a straight line, and the deviations from a straight line are the errors, either constant or variable, and from the observations the constant errors can, of course, be eliminated. Two methods of securing a simultaneous rupture of the primaries have been followed. One plan consisted in wrapping all the wires round a small magazine of fulminate of mercury, and exploding the fulminate. The other consisted in collecting the whole of the wires on a small screen close to the muzzle of a rifle, and cutting them by means of a flat-headed bullet. Both methods have given excellent results. . . .

The powders which we have experimented on may be divided into four classes—1. The old quick-burning, violent powders, such as R. L. G. and L. G.; 2. Pellet Powder; 3. Pebble Powder; and 4. Prismatic Powder; any of the three last classes are superior to the first.

I have arranged on the diagrams exhibited curves intended to illustrate the differences between three of the classes of powder I have been describing, and in each case I have selected an example which I believe to be as nearly as possible a type of the class. For the purpose of comparison they are all taken from experiments with the 10-in. gun.

Our old rule of proof for powder, that of the eprouvette mortar, seems, with our present lights, to be specially designed to produce in powder those qualities whose absence we most desire. Pellet powders are regular cylinders formed in moulds, while pebble powders are tolerably regular lumps of powder cake, about the size of large pebbles. . . . The prismatic differs considerably from these in being a less dense powder, and possessing the property of lighting with extreme slowness. R. L. G. is a quick-lighting and extremely quick-burning powder; pellet and pebble are quick-lighting, slow-burning powders; and prismatic is a slow-lighting and quick-burning powder. It is probable that the prismatic powder owes its extreme slowness of lighting to the deposition of a heavy coating of saltpetre, due to the moisture present in the process of manufacture.

Although we find that almost inappreciable differences in the manufacture cause occasionally great differences of action when the powder is submitted to the test of firing, we are able to point to several causes which are of the greatest importance in modifying the behaviour of the powder in the gun. These points are—1. Specific gravity; 2. Length of time during which the component charcoal has been burned; 3. Degree of moisture employed in manufacture; 4. Hardness; 5. Size of grain.

Much less time was taken by the shot in the earlier parts of its motion in the case of R. L. G. and pebble than in that of prismatic. It may be interesting to mention that the total time taken by a projectile, when fired with a battering charge, to reach the muzzle of a 10-in. gun is about the one-hundredth part of a second. The velocities at each point of the bore, deduced from these time curves, were also exhibited.

The pressures indicated by curves obtained from the chronoscope indications were compared with the corresponding indications with the crusher gauge. They are as follows:—With the pebble, pellet, and prismatic powders, under ordinary circumstances, with ordinary or battering charges of the service and with service vents, the pressures indicated by the crushers placed in the powder chamber do not differ materially from one another, and any of them, or the mean of the whole of them, agree tolerably closely with the maximum pressure indicated by the chronoscope. But when we come to R. L. G. or L. G. powders (the old quick-burning powders), a striking difference manifests itself; not only do the pressures in R. L. G. differ very materially from the indications given by the chronoscope, but they differ widely from one another. . . . We are therefore met in the case of the destructive powders with difficulties which do not exist in the case of slow-burning powders, and as we are compelled to admit that some of these pressures are entirely local or confined to certain portions of the gun, we give the following explanation. Robins obtained high local pressure by placing the musket-bullet at some distance from the charge. This phenomenon doubtless is due to the inflamed gas, vapours, or other products of the combustion of the powder attaining a very high velocity before encountering the resistance of the bullet, and the reconversion of the *vis viva* into pressure accounts for the intense local pressure that Robins observed. The local pressure we have observed can be similarly explained. The *vis viva* of the products of combustion of the first portion of the charge ignited is in like manner converted into pressure at the seat of the shot; and as we know that the rapidity of combustion of powder is enormously accelerated by the tension under which it is exploded, it is possible that this pressure may be increased by a violent disengagement of gas from the unconsumed powder at the seat of the shot. The time during which this abnormal pressure is kept up must be exceedingly minute, even when compared with the infinitesimal times we are considering; for we find the chronoscope pressure, which may be regarded in the case of these "poudres brutales" as representing

the mean of pressures of a violent oscillatory character, hardly altered at all, even although the local pressures—as, for instance, when the rear vent is used—are increased fifty per cent. Other indications lead to the same conclusion; but it is worthy of remark that, when violent local pressures are set up, waves of pressure, so to speak, appear to sweep from one end of the inflamed gases to the other, and to continue more or less during the whole time the shot is in the bore. . . .

To one difficulty I must allude. In the quick-burning powders, at all events, it seems to be certain that all, or at least all but a very trivial quantity, of the powder is converted into gas by the combustion of the powder before the projectile has been materially moved from its initial position. This being the case, how are we to account for the great loss of work which results when, under ordinary circumstances, a charge is ignited from the rear vent? This loss is very variable, but in one instance in our own experiments the work realized in the shot was reduced from 78 foot tons to 58 foot tons per lb. of powder. The cause of this great loss of work, in an instance where it is difficult to believe that any quantity of powder can have escaped ignition, may, perhaps, be sought either on the hypothesis that under this peculiar mode of ignition the products of combustion differ materially from those arising under ordinary circumstances, or, as heat plays so important a part in the pressure of fired gunpowder, it may possibly be surmised that with the rear vent a much greater waste of heat has resulted than in the case of the service vent. I believe it is generally assumed that the loss of work arising from the heat communicated to the gun is altogether insignificant. This is, however, not so. Careful experiments were made on this head some years ago in Italy with rifles, the rifles being fired under three conditions—*viz.* with the bullet as usual, the bullet very considerably removed from the charge, and with no bullet at all. The results were that in all cases the heat communicated to the barrel represented considerably more than one-third of the total work developed, according to Bunsen and Schischkoff, by the combustion of the powder, being greatest when the ball was placed at some distance from the charge, least when the rifle was loaded in the ordinary manner.

The loss of heat would be very different in the case of the large charges with which we are dealing, but it is still much too large to be neglected, and it is certain that where the wave action, to which I have so often adverted, is set up, there is always a considerable loss of useful effect.

Collaterally with the researches of the committee on the action of gunpowder in guns, I have made at Elswick a series of experiments on the tension of the gases in closed vessels, and have been so fortunate as not only to determine the tension of the gases at various densities, but have exploded charges filling entirely the chambers of closed vessels, and have altogether retained, and, by means of a special arrangement, discharged at pleasure the gaseous products of combustion.

The inflamed products were confined in a chamber by means of a gas check; the pressure was determined by means of a crusher arrangement, and the charge was exploded by means of one of Mr. Abel's fuzes. The current passed through an insulated cone, which, the moment the charge was fired, destroyed the insulating material and effectually closed the passage. The details of one or two of these experiments will be interesting to you. When we first made the arrangement for confining the powder absolutely, I thought that the best method of stopping the escape of the gas was to make a steel vent, closing it with a gun-metal plug fused with tin. This arrangement was apparently successful. When I had just got up to the cylinder, and was stooping down to feel its heat, the charge suddenly made its escape with considerable violence. When the cylinder was opened for examination it was found that the escape of the gas was due to the heat of the explosion having melted the tin between the conical plug, and through the melted tin the gas readily escaped. Another most remarkable occurrence was noted in the examination of this cylinder. On taking out the crusher apparatus, I found that a portion of the solid steel projecting into the charge had been melted, and apparently run; also the head of a hardened steel screw had evidently fused. These effects were produced in 32 seconds. By way of comparison I put, for 37 seconds, into one of the hottest of Siemens' regenerative furnaces, at a temperature probably of about 3300° F., a similar piece of steel. It was raised only to a heat of about 180° F. The temperature of this fusion may have been seriously affected by chemical changes through which the fused metal may have passed.

In another experiment I determined the tension of three-quarters of a pound of R. L. G. powder, completely filling the chamber in which it was fired, and having no escape whatever, to be about 32 tons on the square inch. For the purpose of this discourse I made a similar experiment with F. G. and pellet. The results were completely successful. The gas was entirely confined. In the first case, when I got up to the cylinder it was making a singular crepitating noise, due probably to the sudden application of great internal heat. The temperature of the exterior of the cylinder rose rapidly to 111° F., and then remained nearly stationary for some time. I then let the gases escape, which they did with a sharp hissing noise, rising to a scream when any obstacle was placed on the orifice. With the escaping gases there was not the slightest appearance of smoke, vapour, or colour of any kind. The pressure indicated by the F. G. was 37 tons on the square inch, or about 5600 atmospheres. Here, in these sealed bottles, are the solid residues of combustion from the F. G., and also from pebble. In each cylinder had been placed platinum wire and foil of different degrees of thickness. These have disappeared, and I am unable to say in what state they now are, until the residues have been examined.

I look upon the success of these experiments as being of great

importance. Not only, with the assistance of my friend and colleague (on the committee), Mr. Abel, so well known for his researches in explosive substances, shall we be able to determine the various products of combustion when the powder is fired at its maximum pressure, but we shall be able to determine whether any, and if so what, change in the products is due to combustion under varying pressure; we shall also be able to determine the heat of combustion, and solve other important questions. . . .

The practical conclusions to be deduced from the investigations forming the subject of this discourse may be arranged as follows:—

1st.—The maximum pressure of fired ordinary gunpowder, unrelieved by expansion, is not much above 40 tons to the square inch.

2nd.—In large guns, owing to the violent oscillations produced by the ignition of a large mass of powder, the pressure of the gas is liable to be locally exalted, even above its normal tension, in a perfectly closed vessel, and this intensification of pressure endangers the endurance of the gun, while detracting from the useful effect.

3rd.—Where large charges are used, quick-burning powder increases the strain upon the gun without augmenting the velocity of the shot.

4th.—The position of the vent, or firing point, exercises an important influence upon the intensity of wave action; and in further enlarging the dimensions of heavy guns we must look to improved powder, and improved methods of firing the charge, so as to avoid as much as possible throwing the ignited gases into violent oscillation.

5th.—In all cases it is desirable to have the charges as short as possible, and the cartridge so lighted as to reduce the run of the gas to the shortest limit.

[A. N.]

WEEKLY EVENING MEETING,

Friday, March 17, 1871.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

J. NORMAN LOCKYER, Esq. F.R.S.

On the Recent Solar Eclipse.

MY duty to-night, a pleasant one, although it is tinged with a certain sense of disappointment, is to bring before you the observations which were made of the recent eclipse in Spain and Sicily, to connect them with our former knowledge, and to show in what points our knowledge has been extended. In these observations, as you know, we had nothing to do with the sun as ordinarily visible, but with the most delicate phenomenon which becomes visible to us during eclipses. I refer to the *Corona*.

General Notions of the Corona.

Let me, in the first place, show you what is meant by this term, and state the nature of the problems we had before us. I have here some admirable drawings, which I will show by means of the lamp, of the eclipse that was observed in 1851 by several astronomers who left England in that year to make observations in Sweden, where the eclipse was visible. You must bear in mind that the drawings I shall bring to your notice were made in the same region, at places not more than a few miles apart.* The first drawing was made by an observer whose name is a sufficient guarantee for its accuracy—I refer to Mr. Carrington—*and when the sky was absolutely free from clouds.* In the next diagram you will see the corona is changed. The bright region round the sun is no longer limited to the narrow border of light round the dark moon, as seen by Mr. Carrington, but it is considerably expanded. The third gives still a greater extension, although that picture was drawn within a quarter of a mile of the place where Mr. Carrington's was taken. And lastly, we have a drawing made by the present Astronomer Royal, of that same eclipse, *through a cirro-stratus cloud*, as unlike Mr. Carrington's as anything can possibly be. So that you see we began with a thin band of light about the moon,

* Mr. Carrington observed at Lilla Edet, on the Gota River. The Astronomer Royal observed at Göttenburg. The second drawing referred to was made by Pettersen, at Göttenburg; the third by a friend of the Rev. T. Chevallier, at the same place; and I might have added another by Fearnlay, taken at Rixhöft, in which the corona is larger than in any of the others. The series is most instructive. See Mem. R. A. S., vol. xxi.

which would make the corona a few thousand miles high, and we end with a figure which Mr. Airy graphically likens to the ornament round a compass-card, and which gives the corona a height equal to about once and a half the sun's diameter.

I will next bring before you some drawings made during the eclipse of 1858, which was not observed in European regions, but in South America by two first-rate observers—one, M. Liais, a French astronomer, who was stationed at Olmos, in Brazil; the other, Lieutenant Gilliss, who was also there as a representative of the American Government, and observed some thousand miles away in Peru.

I will throw on the screen the appearances observed by these gentlemen, and I think you will acknowledge the same variations between their results, as to degree, while in one case we get a perfectly new idea of the phenomena—a difference in kind. I would especially call attention in the Olmos drawing to those extraordinary bundles of rays of wonderful shapes, which you see are so much brighter than the other portions of the corona. Such forms have been seen in other eclipses, but they are somewhat rare. The drawing made by Lieutenant Gilliss bears the same relation to that made by M. Liais as Mr. Carrington's did to the Astronomer Royal's; so that we may say that we not only get variations in the dimensions of the corona as seen at different stations, but that we furthermore get a strange structure introduced now and then in our drawing in regions where absolutely no corona at all exists in the other.

So much by way of defining the phenomena and giving an idea of the eye observations generally.

Let me now attempt to show you how the phenomena observed in the last eclipse bear upon the results which had been previously accumulated by means of telescopic and naked-eye observations, and by means of the polariscope and spectroscope.

I.—Telescopic and Naked-Eye Observations.

a.—A Part of the Corona is undoubtedly Solar.

The first use I propose to make of the telescopic and naked-eye observations of last year, is to show you a photographic copy of an admirable drawing made by Mr. Brett, who, though unfortunate enough to see the sun only for a very short time, was yet sufficiently skilled to make good use of that brief period. This drawing will bring before you the fact, that even when a large portion of the sun remained unobscured by the moon, Mr. Brett was enabled to see a dim ring of light round the unobscured portion, which since the year 1722 has been acknowledged, beyond all question, I think I may say, to represent something *at the sun*. It was observed in 1722 round the uneclipsed sun, and in more recent times by Mrs. Airy in 1842, and by Rumker 1½ minute before totality in 1860, not to mention other instances. Therefore, we have one observation made during this eclipse, confirming the old one, that in the corona there is a region of some

small breadth at all events which is absolutely solar, and which it only requires a diminution of the solar light to enable us to see. This, then, we may look upon as the known; now let us feel our way gradually outwards.

b.—Rays, or Streamers, are added at Totality.

The drawings made in all the eclipses which have been carefully recorded bring before us quite outside this narrow, undoubtedly *solar region*, observed before totality, as I have shown, and also by Mr. Carrington and by Lieutenant Gilliss during totality in 1851 and 1858, extraordinary appearances of a different order. While in fact we have a solar ring from 2′ to 6′ high, we have rays of all shapes and sizes visible outside, in some cases extending as far as 4°, and in all cases brighter than the outer corona on which they are seen, the rays being different in different eclipses, and appearing differently to different observers of the same eclipse, and even at the same station. Here is a copy of a drawing made by M. Rumker of the eclipse of 1860, and I show it for the purpose of calling your attention to the fact that the two curious rays represented in it belong to a different order of things from those which we see in the rest of the corona. From the beginning to the middle of the eclipse the east rays were the most intense. In the next drawing, which was made by the same observer, you see something absolutely new: and now the western side of the corona is the most developed; we have a new series of bright rays, and altogether it is difficult to believe that it is a drawing made by the same observer of the same eclipse.

The third drawing is a representation of the same eclipse by M. Marquez, who observed with a perfection of minute care which has scarcely ever been equalled: I bring it before you to show that the rays he saw were altogether differently situated. We may conclude then that the rays, although extremely definite and bright—as bright or brighter than the other portions of the corona which are visible before totality, they being *invisible* before totality—appear different to different observers of the same eclipse, and to the same observer during different *phases*.

c.—They change from Side to Side.

I have already said that M. Rumker observed that from the beginning to the middle of totality the rays on the *east* side of the sun were longest and brightest, and that from the middle to the end of totality the rays on that side of the sun where the totality ended were longest and brightest.

We will now carry this observation a step further, by referring to three drawings made by M. Plantamour in the same eclipse, that of 1860. In the first drawing we have the beginning of the total eclipse as seen in the telescope: with the naked eye naturally we should get the sun disappearing at the east or left-hand side, the moon moving from west to east; in the telescope things are reversed, and we have

it right instead of left: and here we have the same thing that M. Rumker observed, namely, that when the eastern limbs were in contact bright rays (M. Plantamour saw three) were visible on the side at which the contact took place. When the moon was half way over the sun, *two* rays of reduced brilliancy were observed on that side, not necessarily in the same position as *those* first observed, but one of *these* has been abolished altogether; and on the other side of the sun, where totality was about to end, we have three rays gradually suggesting themselves: at the end of totality the rays visible at the commencement are abolished, and now instead of them and of those seen at the middle of the eclipse, we have a bran new set of rays on the side of the moon from whence the sun is about to emerge.

This observation I need hardly say is of considerable importance in connection with the fact that from the year 1722 almost every observer of a total eclipse has stated that there is a large increase of brilliancy, and an increase of the size of the corona on the side where the sun has just been covered, or is just about to emerge.

Now, what was there bearing on this point in the recent observations? I have here three drawings, which, though roughly done, you will see are of great importance side by side with those of M. Plantamour. These are drawings which have been sent in to the Organizing Committee by Mr. Gilman, who lives in Spain, and who took considerable interest in the eclipse, and sent the results of his observations to England with the eclipse party when they came home; and it is of importance that you should see everything that Mr. Gilman has done. If you agree with this explanation of the square form of the corona, which was observed in Spain this year, it will explain the quadrangular form observed in the corona in a good many previous eclipses. Mr. Gilman says that at the commencement of totality—let me remind you, the commencement was determined by the disappearance of the sun at the east limb of the moon, which is east in Mr. Gilman's drawing, as he was observing with the naked eye — the commencement, he says, was determined by the corona flashing out very much like a capital D. You see on the black board exactly the outline, and you will at once mentally associate one half of the diagram with the rays observed by M. Plantamour, and the other half in which there is a nearly perfect ring of light round the moon, with the corona observed by Mr. Carrington all round it in a cloudless sky. At mid-eclipse Mr. Gilman also observed the corona, sketched out its outline carefully, and found rays coming out on the opposite side, adding themselves on to the perfect ring first seen there. Opposite the two salient angles he observed at the commencement of totality—represented by the top and bottom of the upright stroke of the capital D—there were two others; *the corona now appeared square*, and then, just before the end of totality came on, the two corners first seen were observed to disappear altogether, leaving nothing but a perfect ring, and where, at the beginning of the eclipse, nothing was seen but a perfectly round ring, the two exactly similar forms on

the opposite side shot forth, and you got a D reversed (Ɑ). Mr. Warrington Smyth, who drew a square corona, saw the light flash out into the corona before the end of totality, and believes that all the angles of the square were not visible at one and the same time.

Here, then, you have observations of exactly the same character as those of M. Plantamour, to which I have referred. In the drawings of both are shown the inner part of the corona, which you saw growing in the observations of 1851, to which were added the strange forms observed in 1858. You have these strange variations positively growing at the same place and the same time, in the same and in different eyes. Obviously there must be very much that is non-solar, call it personality, atmospheric effect, or what you will, connected with it. We have added to the stable the unstable. The question is, to what is this unstable portion due?

d.—They are very variously represented.

I will now refer to other drawings of the late eclipse, which were made in Sicily. For some reason or other, which I do not profess to understand, the corona, which appeared in Spain to be square, and to Mr. Gilman like a D at the beginning, and like a D reversed (Ɑ) at the end,—to all those with whom I have conversed who saw it in Sicily, it appeared as round as you see it here, in this drawing made by Mr. Griffiths; and, instead of being square, we had sent to us all sorts of pictures, a large number of them representing a stellate figure. Here is a drawing made by a Fellow of the Royal Society, on board one of Her Majesty's ships (the 'Lord Warden') which were trying to save the poor 'Psyche' at Catania. In this we have perfectly regular rays drawn from every region of the sun, some long, some short, but similar rays are almost invariably opposite each other; but in the interior, inside these rays, the corona is just as it was observed by Mr. Griffiths at Syracuse. I now show you a drawing made by an American gentleman at sea, between Catania and Syracuse, with one ridiculously long ray, a ray as long as was seen by Otto Struve in 1860. Other drawings were made, even on board the same ship, so unlike each other, and so bizarre, that I need only refer to them as showing that there at all events must be some personality. We have then to account for the variations between the observations made in Spain and those made in Sicily. I regret that we have not a third order of difficulties to contend with, as doubtless we should have had if observations had been made by Mr. Huggins' party in North Africa.

e.—The Rays are accompanied by a Mass of Light.

These changes of the rays from side to side are accompanied by, and are perhaps to a certain extent due to, the bursting forth of brilliant light in their neighbourhood, where the limbs are nearest in contact. This was first observed by Miraldi in the eclipse of 1724, and has frequently been recorded since. Mr. Warrington Smyth, to whom I

have before alluded, states that he noticed this in the last eclipse, and the photographs, I think, have recorded it; but as there is some uncertainty on this point, I need only suggest it.

f.—Long Rays are seen extending from the Cusps before and after Totality.

So far I have referred only to the rays visible during totality, but long rays were seen when a crescent of the sun was visible in 1860 and 1868 by Mr. Galton and Mr. Hennessy. Mr. Brett caught the same phenomenon last year; but as the sky was cloudy the commencements of the rays only were seen, appearing like delicate brushes in prolongation of the cusps. These observations are of great value, *as no one for one moment imagines that these rays are solar*, and yet they are very like those seen during totality.

g.—Sometimes Dark Rays, called Rifts, are seen instead of Bright ones.

These rays to which I have referred are, however, not the only kind of rays that are observed. At times are seen, as it were, openings in the corona; the openings being of the same shape as the rays, that is, expanding as they leave the dark moon, and opening more or less exactly as the rays do. Like the rays also they are sometimes very numerous; in other eclipses they are few in number. Let us take the eclipse observed in India in 1868. Several drawings made there showed the corona as square as it was drawn in Spain last year; others as round as it was seen in Sicily; but the eclipse was not observed only in India, it was observed at Mantawalok-Kelee by Captain Bullock, and at Whae-Whan, on the east coast of the Malayan Peninsula, by Sir Harry St. George Ord, Governor of the Straits Settlements. In the former place we had rifts expanding rapidly as they left the sun—one forms an angle of 90°, the sides of another being *parallel*—separating patches of corona, which in some places extends 2½ diameters of the moon from the sun.

At Whae-Whan we are told that at one particular moment of the eclipse "it was noticed that from several points in the moon's circumference darker rays emanated, extending to a considerable distance into space, and appearing like shadows cast forth into space by something not very well defined;" these dark rays afterwards "diminishing."

Now let us pass on to the eclipse of 1869. In two drawings made by Dr. Gould, in which the changes in the bright bundles of rays come out in a most unmistakable way, we get similar rifts, which changed as violently as did the rays; while in another drawing made by Mr. Gilman the whole corona is furrowed by narrow rifts in all regions lying between violet, mauve-coloured, white, and yellowish white rays!

Now, what have we bearing on this point in the recent observations? No rift was *seen* in Sicily; one rift was recorded by the sketchers in Spain, but more than one rift was photographed in both

places. We must remember, however, in thus bringing eye-sketches and photographs into comparison, first that the eye too often in such observations retains a general impression of the whole phenomenon, while the plate records the phenomenon as it existed at the time at which it was exposed; and secondly, that we know that the plates record chemically, while the eye records visually. We are dealing with two different kinds of light.

I will show you two photographs on the screen. Although the lucid intervals were very rare, we were fortunate enough to get one photograph of the coronal regions in Syracuse, and one in Spain. I now show you the photograph made by the American party in Spain. You see here that, probably owing to a cloud, we get a certain amount of light driven on to the dark moon, and you also see the indications of the rifts. This photograph was taken with an instrument with a small field of view, so that the most important parts of the corona were rendered invisible by the instrument itself.

Lord Lindsay, who also photographed in Spain, recorded no rifts.

In the other photograph, taken at Syracuse, the result is better. We have the equivalent of the rift in the photograph I showed you before. The instrument was extremely unsteady, and the definition not so good as it would have been if Mr. Brothers had had a good opportunity of displaying his skill. We get other fainter indications of other rifts here and there, and the question whether these rifts agree in the photograph taken in Spain with those in that taken in Syracuse is one of great importance; and it is to be hoped that before long it will be set at rest. Some observers think they agree; others think they do not.

But there is an important consideration based on that photograph, to which I must draw your particular attention. I have shown you the photograph as it may be thrown on the screen; but in the photograph itself there are delicate details which it is impossible to reproduce. The dark portions in the corona indicated in the copy I have shown you are merely the bases of so many dark wedges driving out into space, like their prototypes in the Indian eclipse. It is Mr. Brothers's opinion, I believe, that all you see on the screen round the dark moon, all that enormous mass of light, nearly uniform in texture, and these beautiful broad rays between the rifts are really and absolutely parts of the solar corona. I confess I do not wish to commit myself to such an opinion. We want more facts, and the *onus probandi* lies with those who insist upon that view, and I have yet to hear an explanation of them on that basis.

h.—The Corona sometimes seems to be Flickering or Rotating.

We now come to the next point. Time out of mind, that is, for the last two centuries, the corona has been observed to be flickering, wavering, or rotating, moving in every conceivable way and direction. In 1652 it was described as "a pleasant spectacle of rotatory motion."

Don Antonio Ulloa remarked of the corona observed in the eclipse of 1788, "It seemed to be endued with a rapid rotatory motion, which caused it to resemble a firework turning round its centre." The terms whirling and flickering were applied in the eclipse of 1860. This extraordinary condition of things was also thoroughly endorsed by the late observations. It certainly exists, and is among the observations we have to take into account. When I saw an officer of one of the ships at Catania, I asked him if he had taken a drawing of the corona. "No," he said. I asked him, "Did you see any rays?" "Yes." "Then why did you not make any drawing of them?" His answer was, "How on earth could you draw a thing that was going round and round like a firework?" This was not the only observation of the kind, and the tendency of such observations I need hardly say is to strengthen a belief in the unstable, and therefore uncosmical, nature of their rays.

Is this variation of light due to the brilliancy of the corona, and the rapid change of the rays, which is one of the results which comes out clearest? In 1842 the brilliancy of the corona was stated to be insupportable to the naked eye. A similar remark was made to me by several of those officers who saw the last eclipse in Sicily.

II.—Polariscopic Observations.

With regard to the polarization experiments, by the kindness of Mr. Spottiswoode I am enabled to show you, in a very clear way, the *raison d'être* of the polariscopic observations made during this and former eclipses; but the polariscopic ground is a wide one, and it is not my intention to cover it to-night.

I have had this arrangement of lamp, reflector and prisms made so that you may see how the polariscope can determine the percentage of reflected light at different angles, and the direction of reflexion. Assume this lamp to represent the sun, let this reflector close to the lamp represent a particle near the sun, reflecting light to us, we shall naturally have the light reflected at a much larger angle than if the reflector were close to the screen representing a particle in our own air. Having this idea of the angle of reflexion in your minds, and the fact that the larger the angle under these conditions the more the polarization, if you take this lamp, as I have said, to represent the sun, and this mirror to represent any particle, of whatever kind you choose to imagine, it is clear, that in order to get the maximum polariscopic effect from that particle, that you must have it so situated that it will reflect light at a considerable angle to the beam coming from this lamp.

Now it is clear, that in order to polarize the beam most strongly, I must place the reflector close to our imaginary sun. If I so place it as to represent a particle in our own atmosphere, the angle will be so small that the polarization of the light will hardly be perceptible.

Here is our sunlight, which we will polarize at as great an angle

as we can, by placing the reflector close to the imaginary sun, and send it through this magnificent prism, which Mr. Spottiswoode has been good enough to place at our disposal; and in the path of the beam I will place an object so that you may determine whether there is polarized light. [Experiment.] You see there is considerable brilliancy in those colours; their brilliancy depending upon the amount of polarization.

Now if, instead of having our reflector close to our imaginary sun to represent a particle in the sun's atmosphere, we place it near the screen to represent a particle in our own, in which case the angle is extremely small, the brilliancy of the colours will entirely disappear. You see it has disappeared. The colours, as colours, are distinguishable, but their brilliancy has gone.

That is the rationale of the polariscopic observations, which have been made on the occasion of the last eclipse with more elaboration than they ever were before. If we found the corona to be strongly polarized, this was held to be a great argument in favour of the corona being a real solar appendage, an argument strengthened if the polarization was also found to be radial. At present, however, a great many of the observations that have been made have not been received, and those that have been received are as discordant as those obtained in former eclipses, and therefore my account is an imperfect one, because I have not had an opportunity of discussing all these observations. Indeed, if I had, I should hesitate to give an opinion on the subject. When Mr. Carrington saw that small corona in 1851, and Mr. Gilliss saw that small corona in 1858, neither of them traced any polarization whatever; but when M. Liais saw that large corona in 1868 which was invisible to Mr. Gilliss, he in his turn saw an immense amount of polarization, which led him to believe that the corona was solar, the whole of it, rays and everything included, and that we had an indication of a solar atmosphere two or three times higher than the diameter of the sun; that is, an atmosphere two or three millions of miles in height. This observation is not in accordance with the general conclusions from the drawings I have shown you; and let me add that the assumption of reflexion at the sun is not without its difficulties, and that we have not yet traced reflected sunlight, even when the strongest polariscopic effects have been observed.

III.—Airy's and Mädler's Conclusions as the Results of the Pre-spectroscopic Observations.

Before passing to the spectroscopic observations I will state the conclusions at which the Astronomer Royal and M. Mädler arrived after the observations of 1860 had been gathered together.

The Astronomer Royal, in a lecture delivered before the British Association at Manchester in 1861, stated that the assumption of an atmosphere extending to the moon explained the observation of Plantamour, which could, he thought, be explained in no other way, and he

held also that the polarization experiments seemed to show the same thing. The Astronomer Royal was content to find the reflexion, which so many now insist must be at the sun, taking place somewhere between the earth and moon.

M. Mädler's verdict is in the same direction, and though he does not perhaps express so decided an opinion, he maintains that the atmosphere plays a principal part in the phenomenon; and after detailing experiments to show this, he remarks of the solar and atmospheric portions, "Both cover each other and unite in one phenomenon, so that the corona is a mixed phenomenon."

I shall shortly show you that the spectroscope, leaving the telescope out of consideration, has taught us that this is true; though I shall not be able to show you that it is the whole truth; we are not yet in a position to do that. Mädler concludes his observations by remarking, "We cannot share the doubts of those who are afraid to surround the sun with too many envelopes; neither do we find anything unnatural in the statement that the sun has as many atmospheres as Saturn has rings; but we gladly admit that we cannot yet say anything positive. We have here a large field of probabilities, and the decision may yet be distant."

We can speak with more certainty now!

IV.—Spectroscopic Observations.

a.—Spectrum of the Corona first observed by Tennant, Pogson, and Rayet.

We now come to the consideration of those observations in which we are aided by a most powerful and our most recent ally, the spectroscope, first used in the eclipsed sun, as you know, in the eclipse of 1868. You all know that in that year the question of the nature of red flames was for ever settled by M. Janssen, Major Tennant, Capt. Herschel, and others, who observed that eclipse in the most admirable manner; but we have nothing to do with the red flames now, we have to do with something outside them.

Now, most of you are under the impression, and it was mine until the day before yesterday, that the only thing we learnt about the corona in the eclipse of 1868, was that its spectrum was a continuous one; and I need not tell anyone in this theatre that the assertion that it was continuous was one that was extremely embarrassing, and implied that we had something non-gaseous outside the red flames, which seemed very improbable to those who know anything about the subject. But some of you will no doubt remember that, besides Major Tennant, who made this observation, we had a French observer, M. Rayet, who gave us a diagram of the spectrum of one of the prominences, and Mr. Pogson, who has now been for some time in India, and is a well-known observer, who gave us, nominally as the spectrum of a prominence, a spectrum with some curious variations from M. Rayet's diagram.

I exhibit a copy of M. Rayet's diagram of the spectrum of a pro-

minence, as he called it. At the bottom is what he considered as the spectrum of the lower portion of the prominence, while in the higher portion, where we get fewer lines, as he considered, is the spectrum of the higher portion of the prominence; the spectrum of the lower portion contains the lines B, D, E, and F, and some other lines, in all nine, while the spectrum of the upper part of the prominence, as he thought it, only contains three lines. It was at first difficult to account for these observations. In the first place, one could not understand the line B being given, because I soon found that the line B was not seen as a bright line in the chromosphere spectrum; it was clearly the line C that was intended. Hence doubt was thrown on the other lines; it seemed as if M. Rayet was wrong about his elongated lines D, E, and F, and probably meant C near D and F. And so it was explained—I am ashamed to say by myself—that there was no particular meaning in these elongated lines except that the spectrum of the prominence some distance away from the sun was simpler than it was nearer the sun, as happens in all prominences, as we may now determine any day we choose to look at the sun by means of the spectroscope.

Now let us hear Mr. Pogson. He gave a diagram showing five lines in the spectrum of what he thought a prominence, and he writes:—"A faint light was seen [in the spectroscope], scarcely coloured, and certainly free from either dark or bright lines. While wondering at the dreary blank before me, and feeling intensely disappointed, some bright lines came gradually into view, reached a pretty considerable maximum brilliancy, and again faded away. Five of these lines were visible, but two decidedly superior to the rest. . . . . The readings of the two brightest were secured. It struck me as strange that these brightest lines should appear at a part of the spectrum not corresponding to any very conspicuous dark lines in the solar spectrum. . . . . [These lines are a little less refrangible than E.] The third line seen in order of brilliancy must have been either coincident with, or very near the place of the sodium line D, but it was much fainter than the two measured, while the fourth and fifth lines were extremely faint." [They were very faint and DOUBLED, and near F. I have seen F give way to a double line in our hydrogen experiments, though I am not prepared to say this is an explanation of Mr. Pogson's observations.]

The fact that we have here the first observations of the spectrum of the sun's corona is one beyond all doubt; and why M. Rayet and Mr. Pogson thought they were observing prominences when they were observing above them, is explained by a remark made by Captain Tupman, of the Royal Marine Artillery, who acted as jackal to Professor Harkness, and picked out the brighter spots of the corona for his observation. Professor Harkness observing the prominence bright lines, said to Captain Tupman, "You have turned the telescope on to a prominence; I want the corona." "No," said Captain Tupman, "I am giving you the corona as well as I can." It was certainly the corona in both

cases. Here you see, dimly and darkly, the first outcome of the spectroscope on the nature of the corona; a record as fairly written as anything at the sun can write it; and I am more anxious to lay stress on these observations, since they have lain fallow for two years, and show the importance of observations, not only in extending our knowledge, but in explaining prior observations; and it is an additional reason for never rejecting an observation. What was, however, dim and dark in 1868 shone out brightly in 1869, thanks to the skill of the American observers of the eclipse of that year.

b.—Laboratory Experiments bearing on these Observations.

But before I proceed to refer to the admirable observations made in America during this eclipse, I wish to introduce you to some work which was commenced in 1868, and has been done quite independently of eclipses. In a lecture which I delivered here about two years ago, I described to you some of the facts observed by the spectroscope in the bright-line region which had been spectroscopically determined to exist all round the sun, and which, as in it all the various coloured effects are seen in total eclipses, I had named the Chromosphere. It was clear that by the new method of observing this without any eclipse, by partially killing, so to speak, the atmospheric light, we got a percentage only of the phenomenon, as the atmospheric light could only be killed by an amount of dispersion which enfeebled and shortened the chromospheric lines; so that although we could say that an envelope of some 5000 or 6000 miles in height existed round the sun, we could not fix this as a maximum limit. Further, when we examined the spectrum of this envelope we got long lines and short lines; and I told how the short lines indicated a low stratum, and how a long line indicated a higher one. To explain this, I will show you an observation made long before the new method was thought of. Even before that time we had abundant evidence of such strata, if we could not determine their nature: we had distinct evidence either of one thing *thinning* out, and then another, or that various substances were situated at different levels, under different conditions; on the first hypothesis, at the extreme outside of the chromosphere the last thing would thin out, and then there would be an end of all things as respects the sun.

I will show you a drawing made by Professor Schmidt of the eclipse of 1851. I do not wish to call your attention to the strange shape of the large prominence, but to the fact, that as the moon passed over this region we get a thin red band, first along the edge of the dark moon, and after the moon had passed over still further we see this red layer, *suspended as it were in the chromosphere*, with a white layer below it. This is the explanation of the long and short lines visible in the spectrum of the chromosphere; in the red layer we have hydrogen almost alone; below, its red light was conquered by other light with bright lines in all parts of the spectrum, and we get white light.

Lord Lindsay tells me he has a distinct indication, written by the sun himself, that in one particular part of the chromosphere, as recorded photographically in Spain, there were three such layers. And over and over again we find recorded white light close to the sun, then red alone, or red mixed with yellow, then violet, and lastly green. And M. Mädler remarks on this very admirably, "The violet band is the link between the prominences and the corona."

Before going further, I will show you the difference in the appearance of what we may term hot hydrogen and cold hydrogen, that is, hydrogen which we drive into different degrees of incandescence by means of the spark. After Dr. Frankland and myself were able to determine that the pressure in these solar regions was small, we came to the conclusion, that outside the hot hydrogen there must be some cooler hydrogen, in order that the phenomena we observed, both in the laboratory and in the observatory, should agree.

I have in this tube hydrogen at a certain pressure, and here we have a coil which will enable us to send a spark through it; you see we get a certain amount of redness in that tube, and if you look on one side or above you will see a sort of bluish-greenish light. Now that redness represents the condition of the hydrogen in the region of the sun where Dr. Schmidt gave us that extremely thin red ring, and the combination of the blue and red would give you something very like violet.

But here I have hydrogen under a different condition. In the tube its rareness is not excessive; but in this globe, of which I am about to speak, you have the nearest approach to a vacuum ever obtained through which a spark will pass; and I beg to call your attention to what will now happen. This globe contains the same chemical element, prepared at the same time as the chemical element you have in the tube, but you see that, so far as colour goes, we have something perfectly different in this case. Now we send the spark through it. I would beg Professor Tyndall, if he will be good enough, to observe the spectrum of this hydrogen in this globe. [Professor Tyndall did so.] You will see that there is one line? [*Professor Tyndall.* Yes.] And a continuous spectrum? [*Professor Tyndall.* And a continuous spectrum.] Cool hydrogen gives us only the bright line F, plus a continuous spectrum, and many of you will know the extreme importance of that observation. It accounts for the F line being observed without the C line in 1868 and last year, and also for the continuous spectrum observed in the Indian eclipse.

c.—The American Eclipse.

When we come from the Indian to the American eclipse with the considerations to which I have drawn your attention, namely, the existence of these different layers due to the different elements and conditions of the same element thinning out, we shall see the extreme importance of the American observations, for they establish the fact that outside the hydrogen layer there was a layer giving only a line in the green, the line which Rayet and Pogson had observed associated with the

hydrogen spectrum and the spectrum of the yellow substance. Here obviously we have, I think, merely an indication of another substance thinning out, in spite of the extraordinary suggestion which was put forward that the corona was nothing but a *permanent solar aurora*.

I need hardly tell you that the idea of a permanent aurora anywhere was startling, and that of a permanent solar aurora more startling still; but what I claim is that during last year's observations we made this very startling idea into a most beautiful fact, namely, that this outer layer of the chromosphere is in all probability nothing more nor less than an indication of an element lighter than hydrogen, although this is not yet absolutely established, for the line is coincident with one of the lines in the spectrum of iron.

d.—The Layers increase very rapidly in Density. Reproduction of the Coloured Phenomena.

Dr. Frankland and myself were early drawn to consider the solar nature of the large coronas, to which I have called your attention, as extremely questionable, even on the supposition of cool hydrogen, because we did not see how, with its temperature and pressure, it could extend very far: and an experiment which I have to make here will probably make that clearer.

We have in these glass vessels hydrogen a little more brilliant now the spark passes through it than that you saw in the globe, because I have been compelled to mix with it a certain amount of mercury vapour. Below, we have at the present moment sodium vapour being generated from metallic sodium in one tube, and mercury vapour in the other. I hope, if the experiment succeeds, you will see that a good many of the coloured phenomena seen in the chromosphere during eclipses may be easily reproduced by such experiments as this; and not only the coloured phenomena but *the increase of brilliancy* accompanied by changes of colour recorded. You can now all see the yellow tinge at the bottom of one tube, and the green tinge at the bottom of the other; and if there were time to continue this experiment by increasing the density of the vapours now associated with the hydrogen, I could make the bottom portion of each tube where the vapours are densest shine out almost like the sun, while the cool hydrogen at the top would remain not more brilliant than it is at present. We should have as it were a section of the chromosphere.

V.—Conclusion.

I will proceed now, if you will allow me, to some of the general results obtained during the last eclipse.

I think that, although the work has been very unfortunately interrupted, still the result has been most satisfactory. By putting together observations here and observations there, I consider our knowledge of the sun is enormously greater than it was a few months ago. For instance, we are enabled to understand the long-neglected

observation of Rayet, and the equally long-neglected observation of Pogson; and we know that outside the hydrogen there is, in all probability, a new element existing in a state of almost infinite tenuity. And we are sure of the existence of cool hydrogen above the hot hydrogen, a fact which seemed to be negatived by the eclipse of 1869.

I think if we had merely determined that there was this cool hydrogen, all our labour would not have been in vain, as it shows the rapid reduction of temperature; but there is more behind. I told you that M. Mädler, in summing up the observations made up to 1860, came to the conclusion that part of the corona was certainly solar, and that whether the outer portions were or were not solar, was a matter of doubt. I do not say that we have settled that absolutely, but we have firm evidence that *some* of the light of the corona is due to reflexion between the earth and the moon. The outer corona was observed to have a rosy tinge over the prominences, and the spectrum of the prominences was detected many minutes above them, as well as on the dark moon. It could not have got this colour *at the sun*, for its intrinsic colour is green, and the red light of the hydrogen supplied at the sun is abolished altogether, is absorbed, and can only reach the corona *at the sun*, so to speak, as dark light.

It is a great fact that we are sure, as far as observation can make us sure, that there is a glare round the hydrogen which gives us the spectrum of hot hydrogen on the corona, *where we know that hot hydrogen does not exist.* Assume the hot hydrogen which gives us the red light to be only two minutes high, the spectroscope has picked it up eight minutes from the sun! The region of cool hydrogen is exaggerated in the same way. We get it where there is no indication of the cool hydrogen existing. And then with regard to the element which gives us the line of the green, we get that twenty minutes or twenty-five minutes away from the sun. Well, no man who knows anything about the matter will affirm that it is certain that the element exists at that distance from the sun.

Therefore I think we have absolutely established the fact that as the sun—the uneclipsed sun—gives us a glare round it, so each layer of the chromosphere gives us a glare round it. That is exactly what was to be expected, and that it is true is proved by the observation—a most important observation made in Spain—that the air, the cloud, ever between us and the dark moon, gives us the same spectrum that we get from the prominences themselves.

Given, however, the layers and elements in the chromosphere extended as far as you will, and apparently increased or not by reflexion *not at the sun*, we have still to account for rays, rifts, and the like. If anyone will explain either Mr. Brothers's photograph or Mr. Gilman's picture of the eclipse of 1869, containing those dark bands starting from the moon and fading away into space, and the bright variously-coloured rays between them on any solar theory, he will render great service to science. But in the meantime I must fall back upon M. Mädler's opinion of 1860, with the addition to it that I have

stated that we have found, at all events, *that some* of the doubtful light is now solar; we have turned the opinion into a fact.

Bear in mind that close to the sun you have a white layer composed of vapours of many substances, including all the outer ones; outside this is a yellow region; above that a region of hydrogen, incandescent and red at the base, cooler, and therefore blue, higher up, the red and blue commingling and giving us violet; and then another element thinning out and giving us green. Take these colours in connection with those which are thrown on our landscapes or on the sea during eclipses, each region being lit up in turns with varying, more or less monochromatic light, and that light of the very colour composing the various layers, each layer being, as I have shown, so much brighter than the outer ones that its light predominates over them. Is it too much to suggest to those who may be anxious to attempt to elucidate this subject, that probably if they would consider all the conditions of the problem presented by that great screen, the moon, allowing each of these layers by turn to throw its light earthwards, the inequalities of the edge of the *globular* moon allowing here light to pass from a richer region, here stopping light from even the dimmer ones, they would be able to explain the rays, their colours, variations, apparent twistings, and change of side? I do not hesitate to ask this question, because it is a difficult one to answer, since the whole question is one of enormous difficulty. But difficult though it be, I trust I have shown you that we are on the right track, and that in spite of our bad weather, the observations made by the English and American Government Eclipse Expedition of 1870 have largely increased our knowledge.

With increase of knowledge generally comes a necessity for changing the nomenclature belonging to a time when it was imperfect. The researches to which I have drawn your attention form no exception to this rule. A few years ago our science was satisfied with the terms prominences, sierra, and corona, to represent the phenomena I have brought before you, the nature of both being absolutely unknown, as is indicated by the fact that the term *sierra* was employed, and aptly so, when it was imagined the prominences might be solar mountains! We now know many of the constituent materials of these strange things; we know that we are dealing with the exterior portion of the solar atmosphere, and a large knowledge of solar meteorology is already acquired, which shows us the whole mechanism of these prominences. But we also know that part of the corona is not at the sun at all. Hence the terms *leucosphere* and *halo* have been suggested to designate in the one case the regions where the general radiation, owing to a reduced pressure and temperature, is no longer subordinate to the selective radiation, and in the other, that part of the corona which is non-solar. Neither of these terms is apt, nor is either necessary. All purposes will be served if the term corona be retained as a name for the exterior region, including the rays, rifts, and the like, about which doubt still exists, though it is now

proved that some part is *non-solar*, while for the undoubted solar portion the term Chromosphere—the bright-line region—as it was defined in this theatre now two years ago, exactly expresses its characteristic features, and differentiates it from the photosphere and the associated portion of the solar atmosphere.

Here my discourse would end, if it were not incumbent on me to state how grateful I feel to Her Majesty's Government for giving us the opportunity of going to the eclipse; to place on record the pleasure we all felt in being so closely associated in our work with the distinguished American astronomers who from first to last aided us greatly; and to express our great gratitude to all sorts of new friends whom we found wherever we went, and who welcomed us as if they had known us from our childhood.

[J. N. L.]

WEEKLY EVENING MEETING,

Friday, March 31, 1871.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Professor Max Müller, LL.D.

On Mythology.

(Abstract deferred.)

GENERAL MONTHLY MEETING,

Monday, April 3, 1871.

Warren De la Rue, Esq. D.C.L. F.R.S. Vice-President, in the Chair.

William Cubitt, Esq.
William Gould, Esq.
Robert Hannah, Esq.
John Henry Mackenzie, Esq.
Rev. John Macnaught, M.A
Joseph Reynolds Masters, Esq.
George Borwick Robertson, Esq. F.C.S.
George Wilson, Esq.

were *elected* Members of the Royal Institution.

The special thanks of the Members were returned for the following Donations to "The Fund for the Promotion of Experimental Researches":—

T. Williams Helps, Esq. (*6th Donation*) £10

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Agricultural Society of England, Royal—No. 13. 8vo. 1871.
American Philosophical Society—Proceedings, No. 80. 8vo. 1870.
Antiquaries, Society of—Archæologia, Vol. XLIII. Part 1. 4to. 1871.
Astronomical Society, Royal—Proceedings, Vol. XXXI. No. 4. 1871.
Benson, W. Esq. (*the Author*)—Principles of the Science of Colour. 4to. 1868.
Manual of the Science of Colour. 12mo. 1871.
British Architects, Royal Institute of—Sessional Papers, 1870–1, No. 5. 4to.
Chemical Society—Journal for March, 1871. 8vo.
Comitato Geologico d'Italia—Bollettini. 1871. Nos. 1, 2. 8vo. Firenze.
Editors—Academy for March, 1871. 4to.
American Journal of Science, Feb. 1871. 8vo.
Artizan for March, 1871. 4to.
Athenæum for March, 1871. 4to.
Chemical News for March, 1871. 4to.
Engineer for March, 1871. fol.
Horological Journal for March, 1871. 8vo.
Journal of Gas-Lighting for March, 1871. 4to.
Mechanics' Magazine for March, 1871. 8vo.
Nature for March, 1871. 4to.
Pharmaceutical Journal for March, 1871. 8vo.
Photographic News for March, 1871. 4to.
Scientific Review for March, 1871. fol.
Grove, W. R. Esq. Q.C. F.R.S. M.R.I. (*the Author*)—Die Verwandschaft der Naturkrafte, von Dr. W. R. Grove: herausgegeben durch E. von Schraper: mit einem Anhang von R. Clausius. 8vo. Braunschweig. 1871.
Meteorological Office—Barometrical Manual. 8vo. 1871.
Meteorological Society—Proceedings, No. 52. 8vo. 1871.
Photographic Society—Journal, No. 223. 8vo. 1871.
Royal Society of London—Proceedings, No. 126. 8vo. 1871.
Symons, G. J. Esq. (*the Author*)—Symons' Monthly Meteorological Magazine, March, 1871. 8vo.
Symons' British Rainfall, 1870. 8vo. 1871,
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, Sept.–Dez. 1870. 4to.
Victoria Institute—Journal, No. 17. 8vo. 1871. Title, &c. to Vol. IV.
Willis, M. A. (*the Authoress, wife of Rev. R. Willis*)—Short Sketch about Washing Linen, and other Tracts. 16mo. 1869–71.

WEEKLY EVENING MEETING,

Friday, April 21, 1871.

WARREN DE LA RUE, Esq. LL.D. F.R.S. Vice-President, in the Chair.

PROFESSOR BLACKIE, F.R.S.E.

On the pre-Socratic Philosophy.

THE history of origins is always interesting and generally obscure; but in the case of the early Greek philosophy a sufficient number of fragments has been saved from the wreck of tradition to enable us to have a clear view of the salient doctrines of the pre-Socratic thinkers, though not certainly an accurate knowledge of the complete organism of their speculation. The pre-Homeric poetry of Greece, of the existence of which in rich abundance there can be little doubt, can be separated from the new organism into which it was worked by the genius of Homer, only by a process more or less conjectural and slippery; but on some of the most interesting and significant utterances of the school of Greek thinkers who flourished in Asia Minor, Magna Grecia, and Sicily for the century and a half that preceded Socrates, we are able to lay our fingers with as much certainty as on the discourses of Socrates himself reported by the pious discipleship of Xenophon; and the fragments which two hundred years ago lay scattered and untested, have in recent times been so laboriously collected, critically sifted, and organically arranged by the diligent, intelligent, and sober-minded workers of erudite Germany, that it is in the power of every fairly equipped scholar to re-create, in a more or less complete form, the main features of pre-Socratic speculation. With the aid of the learned works of Preller, Karsten, Zeller,* to give a rounded completeness to the information on this subject which we gather from Aristotle, Plato, Eusebius, Hippolytus, Clemens, Plutarch, and Stobæus, I have set myself to present in one broad view whatever of most general human significance and scientific interest seemed to shine clearly out from these early speculations; and the result of my labours is the bird's eye of pre-Socratic thinking which the present discourse contains.

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* Karsten: 'Philosoph. Græc. Vet. Reliq.' Bruxelles, 1830. Ritter et Preller: 'Historia Philos. Græc. Rom.' Gothæ, 1869. Zeller: 'Geschichte der Griechischen Philosophie.'

With regard to the whole of this early period of Greek thought two general remarks may be made: *first*, in reference to its locality, that it is more Asiatic than European, the Greek colonies of Asia Minor being its cradle; and *second*, with respect to its character, that in those early times all knowledge, thinking, and feeling was less specialized than at the present day; in such fashion that, if the things known and speculated on were much fewer, the men who knew and speculated on them were more complete. In our time the gulf that separates the scientific from the metaphysical, the imaginative, and the religious man, and all these from the man of business and affairs, is often very great, and practically impassable. The scholar will have nothing to do with physical science; the student of mind ignores matter; the dissector of brain ignores mind; and both seem either unable or unwilling to bridge over the space that separates the special province of cognition from the general domain of human spiritual instincts, aspirations, and emotions. It seems indeed the inevitable tendency of the division of labour, while it improves and multiplies the product, to narrow and to dwarf the producer. The ancient intellect grew up more like a rich leafy tree with many branches, spreading themselves out towards the sun on all sides; the modern man is taught to live only in a straight line, and to extend himself, as J. S. Mill says of Jeremy Bentham, infinitely in one direction. He thus loses the central point of philosophic survey altogether, or at best works out only one idea, which he is inclined to impose on the universal system of things, and cultivating assiduously one side of his nature, cheats himself of the beautiful symmetry and balance of a complete growth. More particularly we must observe that the opposition betwixt physical science and metaphysical speculation, so common in modern times, was not known to the ancients; their physics was always mixed largely with metaphysics; or rather we now are in the habit of treating separately subjects which then had not begun to be even thought of as separate; the man of physical science was at the same time a metaphysical thinker; the materialist was also a spiritualist; and science stood before men, not naked as now with the exposures of a cunning dissector, but festooned with the flowers of poetry and fragrant with the breath of piety. Hence it comes that in Empedocles, for instance, we find that complex combination of physician, poet, priest, and politician, so difficult for us moderns to understand. Nature was constantly speaking to those ancient Greek sages in the language of morals; and morals on the other hand did not disdain to use the language of nature. That which had never been divorced could not be looked on as antagonistic.

It is not therefore altogether true—certainly not to be taken in a strict and literal sense—when we read that Socrates was the father of Moral Philosophy. Pythagoras was a great moralist and a pious man more than a hundred years before him; the so-called seven wise men, as Laertius well notes, were all not merely or mainly speculative thinkers, but active citizens, and often practical statesmen, and so

could not avoid making human life, and the laws of the social organism, one of their principal studies. The social world, in fact, could no more exist without moral philosophy in some shape or other, than the physical world could grow and blossom without the sun; and the mission of Socrates, therefore, was not so much to create a moral philosophy which had never existed before, as (1), to protest against a habit of physico-metaphysical speculation, which wasted itself in vague conjectures, and which in the then state of scientific observation could lead to no practical result (Xen. Mem. i. 14, 15); and (2), by putting the weapons of an exact logic into the hands of thinking men, to teach them how to vanquish scepticism on its own battle-field, and to plant morals, as practical reason, on the high vantage ground of a demonstrative science. This characteristically Greek tendency to identify morals with reason, and right action with true thinking, appears equally in Xenophon's dialogic reminiscences, and in Plato's argumentative dialogues, as the characteristic feature of the Socratic philosophy; and the scientific control of morals by logic is a peculiarity marked enough to draw a strong line of distinction between the gnomic morality which inspired Homer and Hesiod no less than Pythagoras, and the systematically-built-up ethics of which Socrates and Plato were the architects.

In making a rapid summation of the results of the pre-Socratic thinkers, I shall divide them, according to what appear their most distinctive characters, into three classes: the hylozoists, the atomists, and the metaphysical or theological philosophers.

I. By the *Hylozoists* I understand those philosophers who found the ἀρχή or first principle of things in some external, objective, visible, tangible, material element; and yet they were not materialists in the popular sense of the word at all; for they knew nothing of, and could scarcely have considered that idea, so familiar to us, of a dead matter as opposed to mind. With them all Nature was conceived as essentially alive; "all things," as Thales said, "are full of gods" (Diog. Laert. i. 27); hence the technical term hylozoism, or vitalized matter. With this class of thinkers all matter was conceived of as permeated and interfused and moulded by a divine and reasonable power felt to be everywhere present, but spoken of only and mainly under some favourite bodily presentation. What the orthodox Romanist believes to have taken place by a special miracle through sacerdotal agency in the elements of bread and wine consecrated in the Mass, this the Hellenic hylozoist believed constantly to take place everywhere by the eternal miracle of Nature, and specially to be manifested in the element which he looked upon as most entitled to the dignity of ἀρχή, or original principle. The names which fall under this first class are five, *viz.* Thales, Anaximander, Anaximenes, Heraclitus, Empedocles.

Of all these the most popularly known is THALES; though, so far as speculative significance is concerned, his name is to us certainly not the most interesting. The familiarity of his name to our modern

ears is doubtless owing to his historical connection with the famous names of Solon and Crœsus, made public property by the pleasant pen of Herodotus. His calculation of eclipses also fixes him as a notable mark in the history of mathematical science; while his characteristic doctrine that water, or rather fluidity, is the ἀρχή, has perhaps received an accidental advertisement of wide circulation in the well-known initial words of Pindar's Olympian Odes, ἄριστον μὲν ὕδωρ. The philosophical value of this maxim is not very great; yet it confessedly announced a principle which, so far as it goes, has been confirmed and strengthened by the various investigations of modern physical science. From the salt wastes of Central Asia to the compact ice wedges of the Arctic Ocean, the most superficial view of Nature proclaims that, while solidity is almost the only thing that is absolutely dead, fluidity is always and everywhere the cradle of life. The myriad-moving life of which a drop of water is the sphere has long been one of the favourite exhibitions of our microscopic showmen; and physiologists and organic chemists have been eager to expound to admiring popular audiences how much of this "too, too solid flesh," which we call our bodies, is really composed of the liquid dew into which Hamlet wished that it might dissolve. The fecundity of the fishy tribe was another fact of superficial Nature, on which Thales is reported to have founded his doctrine; a fact which the minute observations and calculations of modern naturalists have brought into most significant prominence. Ancient mythology, also, not only in its Hellenic, but in many far-distant forms, agreed in testifying to the great part played by water in the original creation of the world. Homer (Iliad, xiv. 201) sang of an Olympian dynasty older than Jove,—

"OCEAN *the primal father of Gods, and* TETHYS *the mother;*"

and with this the Babylonian tradition in Berosus of primeval "water and darkness," and the Hindoo Avatar of Brahm, as Narayana (Nereus, νερὸ, *Nar*) strikingly agreed.* The physical truth indicated in these mythologies is distinctly stated in the Mosaic record (Gen. i. 3); and if for the Hebrew phrase, "the *Ruach Elohim*," or "*Breath of God*," we were to substitute the "Divine Reason, or λόγος," any appearance of difference would be removed which to superficial observers may now seem to separate the cosmogony of Moses from that of Thales.

A disciple of Thales was Anaximander, and, like his master, a citizen of the flourishing Ionic colony of Miletus. In the doctrine of this philosopher that the ἄπειρον is the ἀρχή we find a certain departure from the general character of the class to which he belongs; for though this ἄπειρον, the *Infinite*, or rather the *Undetermined*, was something external and objective, it differed from the ἀρχή of Thales, and the other teachers of this school, in that it was a something neither

* See my 'Homer,' vol. iv., note to the line quoted.

visible, nor tangible, nor cognoscible by human senses in their ordinary action. The ἄπειρον was conceived as a sort of indeterminate basis of all existence,—something, perhaps, like the protoplasm of a distinguished living physiologist, containing the possibilities of all that may be, but the reality of nothing that is; an element neither water, nor air, nor earth, nor fire, but a common matrix out of which they are developed by a process of internal differentiation. The only value of this Anaximandrian notion seems to lie not certainly in any reality which can be proved to belong to it, but rather in the truly philosophical principle that it involves, that things are not what they seem, and that what the superficial observer calls different things, are, when nicely looked into, often only different states of the same thing. What profound truth lies in this view the frequent presentation under the appliances of modern chemistry of the same body under the liquid, gaseous, and solid forms, most amply demonstrates. Beyond this, the fragments of Anaximandrian speculation possess nothing to interest the modern thinker; except only we must by no means omit to mention his singular anticipation of the Darwinian theory of the descent of man by a process of development from the inferior animals. This doctrine is distinctly stated as belonging to Anaximander by Hippolytus (*Ref. Hær.*, i. 6), and by Plutarch in various places, in words as we shall now translate them.

"Anaximander says that the *first animals* were generated in the *liquid* element, and were enclosed in certain thin *prickly rinds* or *skins*; that as they grew they became more and more consolidated, till at length the shell bursting, they came out into a different form of life." (Plac., Phil. v. 19.)

"Anaximander says that man originally was born (developed) out of different animals; for that, while other animals, the moment they are born, know how to help themselves and procure food, man alone requires a long nursing; and being such as he now is, could not possibly have had his life preserved in the earliest stages." (Apud Euseb., Præpar. Evang., i. 8.)

"Anaximander declares that man originally had his existence as a sort of FISH among FISHES, and that being nursed in a soft element, like creatures that live in the MUD, he by degrees became strong enough to help himself, and being cast out from his original slimy home, took the dry land, and became a terrestrial animal." (*Quæst. Sympos.*, viii. 4.)

This theory, indeed, which has so strangely startled us moderns, was quite familiar to the Greeks, and appears in the well-known account of the Egyptian frogs crawling, half-released, from the slime of the Nile (Ælian, V. H.), and in not a few other quarters. In modern times, also, Lord Monboddo was not singular in directing attention to the probable indications of an original caudal appendage in man; a French writer before him had expatiated largely on the evidence for this fact, and for that other yet more remarkable one, that the sea had at one time possessed the whole dry land—at which

period man, according to Anaximander's idea, was certainly a fish, and could, indeed, have existed in no other form.*

Anaximenes, the next link in this Milesian chain, need not detain us. By laying down the proposition that *air*, not *water*, was the ἀρχή, this thinker certainly made a step in advance; for air is certainly fluidity in its most perfect form: a form also of more subtle penetrative power than water—of more universal diffusiveness, and more essentially connected with all forms of vitality. But beyond this we find nothing in his doctrine of any peculiar significance. Far otherwise is the case with the next representative of this school—Heraclitus. He was a native of Ephesus; and from the manner in which he is constantly spoken of by the greatest of the ancients, it is easy to gather that as a thinker he peers high above his Ionic brethren; and disputes with Pythagoras and Parmenides the claim to being the most important name in the development of Greek speculation previous to Plato. The surname of σκοτεινός, or "the *obscure*," which he received from the Greeks, may have arisen partly from an original moodiness of disposition, partly from a fondness for the old poetic and figurative style in philosophizing, but partly, no doubt, also from the real profundity of his cogitations. He evidently went with a decided preference into regions where darkness more or less visible must always be the best light which finite minds are capable of receiving. The most distinctive and suggestive of his dogmas are the following:—

(1) His doctrine that πῦρ, or FIRE, is the ἀρχή, is a very decided step in advance both of Thales and Anaximenes; for chemistry has now taught us by many curious processes, what appears no doubt broadly on the general face of Nature, that heat is the power to the action of which both water and air owe their existence. Take away heat from water and it becomes ice; take away heat, and the expansiveness caused by heat, from a gas, and under the influence of strong pressure it becomes *liquid*. Heraclitus therefore proceeded by a true process of induction when he put his finger on heat as that common principle which, by producing fluidity, makes life possible.

(2) But this fire or heat is nothing of a merely material kind, like the caloric, or matter of heat, of which our captains of physical science were once fond of talking. The fire of Heraclitus is a reasonable or rational heat; it is inspired by a λόγος; it is φρόνιμον and φρενῆρες (Hippol., ix. 10. Sext. Empir. *adv. Math.*, vii. 127); it works by a divine necessity, which is the cause of order and law in the universe, and on which the validity and stability of all human laws ultimately depend. Mind and Reason, indeed, are diffused everywhere in the universe, and are the common elements in virtue of which a harmonious and coherent whole is rendered possible. Ξυνον ἐστι πᾶσι τὸ φρονεῖν· τρέφονται γὰρ πάντες οἱ ἀνθρώπινοι νόμοι ὑπὸ ἑνὸς τοῦ θείου·

* 'Telliamed; or, Discourses between an Indian Philosopher and a French Missionary;' from the French of DE MAILLET. London: 1750.

κρατέει γὰρ τοσοῦτον ὁκόσον ἐθέλει, καὶ ἐξαρκέει πᾶσι καὶ περιγίγνεται. (Stob., Serm. iii. 84.) To a modern ear this doctrine may probably sound very like Pantheism; and no doubt it is Pantheism, but a sort of Pantheism by no means like some of our modern Pantheisms, full cousins to Atheism. It is rather like the Pantheism of Spinoza, which, if it is to receive a peculiar name, ought, according to Hegel, rather to be called *Acosmism* than *Atheism*. It annihilates the world by concentrating all the significance of the term existence in the idea of God.

(3) The divine rational energy, called πῦρ, is essentially self-motive, and never at rest. Πάντα ῥεῖ—*all things are in a perpetual flux*, so that a flowing river is the most proper type of universal existence. (Plato, Crat. 402A).

> "*Nothing to hold itself is strong,*
> *But all things, like a river,*
> *Whirl along and swirl along,*
> *And bubble along for ever.*"

(4) But, though the motion of existence in the cosmos is shapeless and ceaseless, it is in one important respect not like the course of a river; it is not a motion directly forward and onward; but it is a motion forward and backward, with recurrent strokes like the vibrations of the string of a lyre or a bow.* This implies a sort of self-contradiction inherent in the very nature of things. Life is manifested by the assertion of contraries: health, beauty, truth, and rightness of all kinds consist in a balance of opposites. This doctrine Heraclitus felt so strongly, that he did not hesitate to say that πόλεμον εἶναι πατέρα καὶ βασιλέα καὶ κύριον πάντων (Plut., Is. Osir. 48) "*War is the father, and king, and lord of all things;*" and in putting forth this, to us strange sentiment, he no doubt in somewise anticipated the famous maxim of Spinoza, adopted by Hegel—*Omnis affirmatio est negatio.*

(5) Heraclitus further taught that at the bottom of this constant vicissitude, which we call life and the world, there is a something which never changes—an absolute sameness in the midst of relative otherness; so that what we call death is really only another form of life; and the life of any one thing always grows out of the death of some other thing.† This side of his speculations, if carried out, might have formed the basis for a doctrine of permanency, which would have been the natural and proper counterpoise to the sensuous scepticism that was only too easily deduced from his favourite doctrine of flux; but it was reserved for the philosophers of the abstract and strictly metaphysical school to give due emphasis to this essential element of the Cosmos.

The fifth and last doctor of this class was Empedocles of Agri-

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* Παλίντονος ἁρμονίη κόσμου ὅκωσπερ λύρης καὶ τόξου. Plut. Is. Osir., 45.

† Ὁδὸς ἄνω καὶ κάτω μίη καὶ ὠυτή. Hippol. ix., 10.

gentum, a man of noble family, lofty character, great talent, and various accomplishment, of whose poems, περὶ Φύσεως and καθαρμοί, considerable fragments are preserved, commented on in Karsten's collection and Preller's history of Græco-Roman philosophy. Important as was the figure which he played in his native country, in the curiously compound character of a physician, a priest, and a politician, he does not seem to have said much on philosophy that had not been already said by his predecessors. Like Plato, naturally a poet, and a man of a constructive imagination, he seems to have wished to combine into one rich whole the speculations of his predecessors, partly disguised under different names. The WATER, AIR, and FIRE of three of his predecessors he adopted, and with the very obvious addition of EARTH to them, he constructed the universe out of our familiar four elements, which he called ῥιζώματα, constantly submitted to the influence of the two adverse principles, Φιλία and Νεῖκος—LOVE and STRIFE. This was evidently nothing but an old mythological expression for the doctrine of balanced contraries laid down by Heraclitus; and the only thing worth saying about it is that it suggests to modern associations more directly the attractions and repulsions of physical science made so familiar to us by the establishment of chemistry as one of the most fertile fields of discovery. The alternation of life and death, of construction and destruction, of organization and dissolution, caused by the eternal action of Νεῖκος and Φιλία, comes strongly out in some lines preserved by Simplicius (*Phys.*, 34A), which I translate as follows:—

> "*First from the* MANY *grew the one; and then*
> *The* ONE, *resolved into its parts, again*
> *Became the* MANY; *thus the changes run*
> *Of all that lives and dies beneath the sun;*
> *Now* LOVE *unites their elements; and they*
> *Chained into one a balanced whole display,*
> *Now by strong* HATE *and* STRIFE *their parts resile,*
> *And into ruin drops the stately pile.*"

His fragments are remarkable for a very strongly pronounced materialism, as we would call it, not, however, without a distinct recognition of the Pythagorean λόγος, which happily withdraws him from the companionship of that gross school of thinkers who, as Plato says in the 'Theætetus,' believe only in their fingers; at the same time he asserts in language which, if used by a modern writer, would cause him to be set down as full brother to Helvetius, that all αἴσθησις is φρόνησις, and especially that the seat of φρόνησις is in the blood. He also shows a decided tendency to break down the wall of partition between man and the lower animals; nay, he asserts that intelligence belongs to plants as well as to animals; and that man in his present state is in the fourth degree of gradual ascent from a less complete and more dependent to a more complete and less dependent state of existence (Plut., Plac. Phil., v. 19). He thus shares with Anaxi-

mander the glory or the folly, as it may turn out, of the development theory of animal life, so attractively put forward by Darwin.

II. Of the second great class of the pre-Socratic philosophers the distinguished head was Democritus of Abdera. He was the greatest naturalist and traveller, and the most encyclopædic man of his day—a sort of Humboldt of the fifth century B.C., and a precursor of Aristotle; and it is as a man of science and universal knowledge rather than as a philosopher that he must take rank in the history of intellect. The doctrine of atoms which he originated has no doubt a certain philosophical value, though assuredly not a very profound one; for it is easy to see that all composite masses can be broken down, dissipated, and resolved into certain infinitesimally small atoms or molecules, which must be regarded as their ultimate elements. But when the sage of Abdera, by adding to this primitive material fact the idea of mere force, conceived himself to have given an explanation of the curious structure of the reasoned system of things which we call the cosmos, he manifestly did not advance, but rather made a great step backward in the physico-metaphysical speculation of his predecessors. They had always explicitly or implicitly asserted the presence of an indwelling λόγος, or reason, as the soul of the world: this was a living and a dynamical conception, which contained, without attempting to explain, the great mystery of a miraculous divine centre of existence; but they never attempted mechanically to construct the universe out of mere variously-formed atoms, brought together by blind forces and fortuitous collisions. The philosophy of Democritus is in fact pure Atheism—that is, pure nothingness and nonsense; for a reasonable world can never be conceived as the possible result of an unreasonable cause. If the maundering of a madhouse were to go on to all eternity, it could never produce sanity; if the babblement of a host of children were to go to infinity, it could never stumble into the demonstration of a single proposition in Euclid; and so evident is this that one cannot but feel seriously inclined to doubt whether, after all, Democritus, had we been able to submit him to a cross-examination, would have pleaded guilty to the inanity of the Epicurean Atheism of which he stands historically as the father. Certainly with regard to Protagoras, Diagoras, and a few other Greeks of this period, who are said to have professed open Atheism, it never can be proved that in their negation of all theology they meant to do more than protest against the existence of such superhuman beings as the anthromorphic gods, which they found as objects of popular worship in their own country; and the writings of some modern speculators, such as David Hume, teach us to believe that a subtle thinker may puzzle himself in theory with arguments tending to sap the belief in a reasonable Cause, and yet refuse to acknowledge the consequences of his argument when it lands him in the positive profession of blank Atheism. But however this be, the thinker who constructs a theory of Nature without God can have no pretensions to the name of philosopher; the very fact of his ignoring the only

possible explanation of the existence of a reasonable system of things is a proof that he has given up philosophy in despair. He may be a man of subtle thought, of curious science, of universal knowledge; but a philosopher, or an expounder of ultimate truth, he cannot possibly be; for the world which it was his business to explain he not only leaves a riddle and a mystery as he found it, but makes it absolute nonsense. We need not be surprised, therefore, that as a philosopher Democritus exercised no influence on the two great schools of Greek thought which the next century saw founded in Athens: the cold, sharp, analytic intellect of Aristotle, and the lofty, constructive genius of Plato stood equally apart from a pseudo-philosophy which was content to float about blindly in a blind vacuity, and to reason upon postulates which implied the absolute denial of all reason. No philosopher could prosper in reasoning Greece of which λόγος in some shape or other was not the centre.

III. We now come to the third and last and most distinguished class of early Greek thinkers, *viz.* those who taught that the ἀρχή was not a thing objective, visible, or tangible, or appreciable by the senses in any way, but an inner, invisible, moulding, and formative principle, which we call mind, or at least some function of mind, and which cannot be conceived as objective or material. This philosophy of course gave the direct contradiction to all atheism, and therefore it may be properly called theological; it is also rightly termed metaphysical, because it finds the ἀρχή or fundamental cause of all things in something behind and beyond that beautiful and various outside which we call the world. Now no doubt Thales, Heraclitus, and Empedocles, as we have indicated, all believed in this metaphysical cause of everything physical as much as Pythagoras, Xenophanes, and Anaxagoras; but they presented this metaphysical cause always under the mask of some physical element; and the key-note of their thinking directed attention in a marked way to something external. But when Pythagoras, the great founder of the theological philosophy, declared the first principle of all things to be Number, he brought the philosophical mind into an entirely new position, turned as it were the obverse side of the coin, and showed an image and superscription there altogether different from that which had been previously exhibited. By ἀριθμός of course was meant, not merely multitude or succession of units, but *calculated number*, *relation*, *proportion*, and all that curious construction of nicely-measured spaces on which symmetry in architecture, harmony in music, and the whole practice of the fine arts depend. In announcing this principle Pythagoras saw deeper into the nature of the cosmos than any of the pre-Socratic philosophers; he saw that the world is a world simply because it is a κόσμος, or an *order*; and order always depends on calculation, on the arrangement of units according to certain relations of number. But the relations of number are a thing purely abstract and intellectual; they are the product of thinking; they are not derived from the senses; animals see an infinity of things, and see them often with a more keen and sharp observation than men;

but animals have no calculation; the cleverest monkey is much farther removed from the comprehension of the simplest arithmetical proposition than the stupidest man, for the one is separated by a gap, the other removed only by a step or steps from all abstract notions. It is plain therefore that by asserting number as the ἀρχή, Pythagoras was only defining the method of operation of MIND, or λόγος; and what he called NUMBER was only another name for what our modern men of physical science call LAW. For that law is altogether a matter of calculation any one may see in the constant practice of mechanical philosophers, astronomers, and chemists; the laws of nature are only the regularly acting forces of nature; and all regularly acting forces are necessarily either directly or indirectly the product of mind; as a steam engine, for instance, however often multiplied, is always and everywhere the product of the calculating intellect of James Watt, propagated and perpetuated through countless imitators. In assigning this high position to the mathematical element in the constitution of the universe, Pythagoras certainly deserves to be reverenced as the great prophet and anticipator of our modern scientific processes; what he demonstrated with a fine preference in the case of the musical scale, is now demonstrated in everything; not only the motions of the stars and the arrangement of leaves in plants, but the very winds and storms are made the subjects of calculations. Unless indeed it be in the flow of emotion, the flash of wit, the play of fancy, and the fervour of passion, it is difficult to find now a domain that can boast its freedom from the wide control of calculated Number.

As in reference to the mundane system, the Pythagorean assertion of ἀριθμός necessarily implied the unity of the plan of the world, and its existence as the product of a calculating architect, so in reference to the social system the same idea brought into prominence the principle of order, and authority, and subordination, that at the present day gives its leading features to what are called Conservative politics, as opposed to the politics of a free and equal individualism. Pythagoras accordingly preceded Plato in the constitution of a republic mainly on the principle of order; and in one respect certainly far excelled his brilliant successor; for the constitution which the Athenian projected remained a dream upon paper, while the community established by the Samian was a realized fact of considerable social importance for a season. In the establishment of this community indeed he comes nearer to the founders of the great moral society of the Christian Church than any ancient philosopher; it appeared, however, that in this he occupied ground which philosophy was not competent to maintain; for the Pythagorean societies in South-Eastern Italy, being oligarchic in their character and operation, came into collision with that democratic element which was always so potent in the Greek colonies, and being cast out lost their organic consistency. This great failure no doubt gave the hint to Plato to content himself with founding a school at Athens, not a community; for it is only upon very rare occasions that political parties have ventured to interfere with the ventilation of unapplied speculations in the schools.

Less popularly known than Pythagoras, but scarcely of less significance in the history of thought, is Xenophanes of Colophon, who stands chronologically between the Samian and Heraclitus. As the founder of the Eleatic School, he, with Parmenides, stands forward as the champion of the principle of unity in the universe, adding the necessary counterpoise to the doctrine of flux and mutability, on which, as we have seen, Heraclitus so moodily rung the changes. How to bridge over the gap between the one and the many—to apprehend how the ἓν became πολλά if we start from the one, and how the πολλά was worked into the unity of a consistent whole, if we are to start from the many—that of course was, and is, the great problem of all metaphysical and theological thought. To solve this problem, the Eleatics took the method of Spinoza; they asserted the one as the only really existent, and allowed the many to shift for themselves as they best might under the name of appearances, modifications, illusions (the Hindoo *Maya*), and whatsoever you please, of the nature of fleeting, ephemeral, and without independent root and enduring reality. God only exists; all created things are mere ephemeral phantoms and phenomenal illusions. As assertors of the self-existent ἓν, the Eleatics were naturally opposed to that breaking down of the divine nature into a multitude of discordant personalities which they beheld in the popular polytheism of their country; and it is one of the great merits of Xenophanes that he anticipated Plato by a century and a half in publishing a rigorous protest against the contradictions, puerilities, and immoralities of the Homeric theology. Whether the boldness of his utterances in respect to the popular theology may not have been the cause of his leaving his Asiatic home, and settling, like Pythagoras, in Southern Italy, we have no means of knowing; the following declarations, however, which I translate from his fragments in Karsten's collection, if they had got beyond the author's private circle, are certainly strongly enough worded to have made the writer's position sufficiently uncomfortable in that part of the world where the presence of St. Paul, five centuries afterwards, raised such a storm of blatant spite and fury among the worshippers of the Ephesian Artemis.

I.

"There is one God, supreme above all gods and men that be;
Not like a mortal thing in shape, nor like in thought is He.

II.

"O vain conceit, to ween that gods like men are born, and show
Our human face, and use our speech, and in our garments go!

III.

"If sheep and swine, and lions strong, and all the bovine crew,
Could paint with cunning hands, and do what clever mortals do,
Depend upon it every pig with snout so broad and blunt,
Would make a Jove that like himself would thunder with a grunt;
And every lion's god would roar, and every bull's would bellow,
And every sheep's would baa, and every beast his worshipp'd fellow
Would find in some immortal form, and nought exist divine
But had the gait of lion, sheep, or ox, or grunting swine.

IV.

"Homer and Hesiod, whom we own great doctors of theology,
Said many things of blissful gods that cry for large apology,
That they may cheat, and rail, and lie, and give the rein to passion,
Which were a crime in men who tread the dust in mortal fashion.

V.

"All eyes, all ears, all thought is God, the omnipresent soul,
And free from toil, by force of mind he moves the mighty whole."

The only other point worthy of remark with regard to Xenophanes is that he seems to have been the first Greek speculator—certainly the first of whom any record is left—who distinctly noted those curious phenomena in the crust of the earth's surface indicative of an early disturbed state of the globe, which, when collected and systematized, have grown up into the interesting modern science of Geology. The passage in which this remarkable notice occurs is in that part of the work of Hippolytus (Refut. Hær., i. 14) which gives a review of the opinions of the Greek philosophers; and here he distinctly says that "shells, and the prints of fishes, and marine animals were found in the rocks at Syracuse, Malta, and Paros; and that this was evidently the result of a revolution of the Earth's surface, when everything was swathed in mud, and the living creatures of the sea left the impressions of their bodies in the soft beds, which afterwards became dry."

The last philosopher whom our present scheme requires us to mention is Anaxagoras, the friend of Pericles, and the precursor of Plato in the complete emancipation of metaphysical speculation from physical symbols. No doubt Socrates in a well-known passage accuses this philosopher of not having consistently applied the intellectual principle of the world which he laid down (Plat. Phæd. 98 B.); but in declaring that the only adequate explanation of the cosmos was to be found in self-existent self-energizing νοῦς or MIND, Anaxagoras certainly made a stride in advance, marking him out as well worthy of the commendation bestowed by Aristotle, that in this matter he spoke like a full-grown man as contrasted with the lisping and babblement of children. There is in fact only a very superficial difference of expression between the νοῦς of Anaxagoras and the ELOHIM of Moses. "In the beginning GOD created the Heaven and the Earth." About the exact meaning of the word "create" (*Bara*), of course our finite intellects always must dispute: if we could understand that word fully, we should be not men, but God: apart from this, however, the βασιλικὸς νοῦς of Anaxagoras and Plato, which is the keystone of Christian faith as well as of the highest modern thinking, is simply another name for Θεός—GOD. We find, therefore, in the ἀρχή of Anaxagoras a natural and beautiful culmination which harmonizes in one significant watchword the philosophy of Greece, the faith of Christendom, and the instincts of a healthy humanity.

Of the sophists as predecessors or contemporaries of Socrates it is not necessary to say anything in this paper. They were not so

much founders of systems of philosophy as disputers about philosophy—corresponding to a certain class of our literary men, who talk and write on all subjects without having any strong convictions on any subject; some of them, indeed, as Gorgias, professing only to be teachers of rhetoric, and others adding to that some superficial instruction in the principles of a worldly morality and a time-serving statesmanship. The philosophy that they taught was generally of a negative and destructive character; and the part which it played in the history of thought was mainly to bring out in full armed strength the rational ethics of Socrates, Plato, and Aristotle. Of course I am fully aware of the attempt recently made by Mr. Grote to plant these gentlemen on a higher and more dignified platform; but great as are the merits of this distinguished writer in reference to the political history of Greece, I cannot but regard his chapter on the sophists as the product of a reactionary feeling, doomed to pass away with the generation which gave it birth.

[J. S. B.]

WEEKLY EVENING MEETING,

Friday, April 28, 1871.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

WILLIAM ODLING, M.B. F.R.S.

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On the Revived Theory of Phlogiston.

Observationem, quam produco, bono jure mihi vindico. . . . Materia hæc ignescens, in omnibus tribus regnis, una eademque existit. Unde, ut e vegetabili in animale, abundantissime transmigrat, ita ex utrolibet horum, in mineralia et ipsa metalla, promptissime omnium transfertur.—STAHLII, Experimenta, Observationes, Animadversiones, CCC Numero.

IN 1781–83, Cavendish showed that when inflammable air or hydrogen, and dephlogisticated air or oxygen, are exploded together in certain proportions, "almost the whole of the inflammable and dephlogisticated air is converted into pure water," or as he elsewhere expresses it, "is turned into water."

On June 24, 1783, the experiment of Cavendish was repeated on a larger scale and in a somewhat different form by Lavoisier, who not only confirmed the synthesis of the English chemist, but drew from it the conclusion—at first strongly contested, then rapidly acknowledged, and since never called into question—"that water consists of

inflammable air united to dephlogisticated air," or that it is a compound of hydrogen and oxygen.

This conclusion, so opposite to his own preconception on the matter, Lavoisier subsequently confirmed by an analysis of water. He found that iron, heated to redness and exposed to the action of water-vapour, became changed by an abstraction of oxygen from the water, into the self-same oxide of iron procurable by burning the metal in oxygen gas,—the other constituent of the water, namely, its hydrogen, being freely liberated.

With the demonstration by Lavoisier of the composition of water began the triumph of that antiphlogistic theory, which he had conceived, in a necessarily imperfect form, so far back as 1772, or before the discovery of oxygen, and had brought to completion by the aid of every successive step in pneumatic chemistry, achieved by himself or by others.

In 1785, the relationship to one another of hydrogen and water being then conclusively established, Berthollet declared himself a convert to the new theory of combustion put forward by his countryman. Fourcroy next gave in his adhesion; and soon afterwards De Morveau, invited to Paris expressly to be reasoned with by Lavoisier, succumbed to the reasons set before him. The four chemists then associated themselves together, and in spite of a strong though short-lived opposition both in England and Germany, succeeded in obtaining for *La Chimie Française* an all but universal recognition.

The principal articles of the new or antiphlogistic theory of combustion propounded by Lavoisier are as follows:—That combustible bodies in burning yield products of various kinds,—solid in the case of phosphorus and the metals, liquid in the case of hydrogen, gaseous in the case of carbon and sulphur. That in every case the weight of the products formed by the burning is greater than the weight of the combustible burnt. That the increase of weight is due to an addition of matter furnished to the combustible by the air in which its burning takes place. That bodies of which the weights are made up of the weights of two or more distinct kinds of matter are of necessity compound; whereas bodies of which the weights cannot be shown to be made up of the weights of two or more distinct kinds of matter are in effect simple or elementary. That inasmuch as the weights of the products furnished by the burning of different combustibles are made up of the weights of the combustible burnt and of the oxygen consumed in the burning, these products are compound bodies—oxides in fact of the substances burnt. That inasmuch as given weights of many combustibles, as of hydrogen, sulphur, phosphorus, carbon, and the metals, are not apparently made up of the weights of two or more distinct kinds of matter, these particular combustibles are in effect elementary; as for the same reason is the oxygen with which in the act of burning they enter into combination. And, lastly, that combustion or burning consists in nothing else than in the union of combustible matter, simple or compound, with the empyreal matter, oxygen—

the act of union being somehow attended by an evolution of light and heat. And except that it would be necessary nowadays to explain how in certain cases of combustion, the combustible enters into union not with oxygen, but with some analogue of oxygen, the above precise statement might equally well have been made by Lavoisier in 1785, or be made by one of ourselves at the present day.

Lavoisier's theory of combustion being known as the antiphlogistic theory, the question arises, What was the phlogistic theory to which it was opposed, and which it succeeded so completely in displacing? This phlogistic theory was founded and elaborated at the close of the seventeenth century by two German physicians, Beccher and Stahl. Having exercised a scarcely disputed authority over men's minds until the notorious defection in 1785, it preserved for some years longer a resolute though tortuous existence, and was to the last defended and approved by our own Priestley and Cavendish—who died, the former in 1804, and the latter in 1810.

The importance attached to the refutation of this theory may be judged of from the circumstance that after the early experiments of Lavoisier on the composition and decomposition of water had been successfully repeated by a committee of the French Academy in 1790, a congratulatory meeting was held in Paris, at which Madame Lavoisier, attired as a priestess, burned on an altar Stahl's celebrated *Fundamenta Chemiæ Dogmaticæ et Experimentalis*, solemn music playing a requiem the while. And the sort of estimation in which the Stahlian doctrines have since been held by chemists is fairly illustrated by a criticism of Sir J. Herschel, who, speaking of the phlogistic theory of chemistry, says that it "impeded the progress of the science, as far as a science of experiment can be impeded by a false theory, ... by involving the subject in a mist of visionary and hypothetical causes in place of the true acting principles." Possibly, however, this much-abused theory may yet prove to contain an element of permanent vitality and truth; anyhow the study of this earliest and most enduring of chemical theories can never be wholly devoid of interest to chemists.

To appreciate the merit of the phlogistic theory it is necessary to bear in mind the period of its announcement. Its originator, Beccher, was born in 1625, and died a middle-aged but worn-out man in 1682, a few years before the publication of the 'Principia.' His more fortunate disciple, Stahl, who was born in 1660 and died in 1734, in his seventy-fifth year, though afforded a possibility of knowing, seems equally with Beccher to have remained throughout his long career indifferent to the Newtonian principle that the weight of a body is proportionate to its quantity of matter—that loss of weight implies of necessity abstraction of matter, and increase of weight addition of matter. Whether or not the founders of the phlogistic theory conceived that change of matter in the way of kind, might equally with its change in point of quantity, be associated with an alteration in weight—and it must not be forgotten what pains Newton thought

it necessary to take in order to show the contrary—certain it is they attached very little importance to the changes of weight manifested by bodies undergoing the metamorphosis of combustion. It might be that when combustible charcoal was burned the weight of incombustible residue was less than the original weight of charcoal—it might be that when combustible lead was burned the weight of incombustible residue was greater than the original weight of metal—this was far too trifling an unlikeness to stand in the way of the paramount likeness presented by the two bodies. For the lead and charcoal had the common property of manifesting the wonderful energy of fire; they could alike suffer a loss of light and heat—that is, of phlogiston—by the deprivation of which they were alike changed into greater or less weights of inert incombustible residue.

And not only were these primitive students of the philosophy of combustion unconscious of the fact and meaning of the relationship in weight subsisting between the consuming and the consumed body, but they were altogether ignorant of the part played by the air in the phenomena which they so boldly and successfully attempted to explain. Torricelli's invention of the barometer and Guericke's invention of the air-pump were both indeed made during Becher's early boyhood; but years had to elapse before the consequent idea of the materiality of air could be domiciled, as it were, in human understandings. And not until more than a century after Torricelli's discovery of the weight of air,—not, indeed, until the time of the great pneumatic chemists Black, and Cavendish, and Priestley, and Scheele, was it ever imagined that the aerial state, like the solid or liquid state, was a state common to many distinct kinds of matter; and that the weight or substance of a rigid solid might be largely contributed to by the weight or substance of some constituent having its independent existence in the aerial or gaseous form. The notion that 100 lbs. of smithy-scales might consist of 73 lbs. of iron and 27 lbs. of a particular kind of air, and that 100 lbs. of marble might consist of 56 lbs. of lime and 44 lbs. of another kind of air, was a notion utterly foreign to the older philosophy. Air, it was allowed, might be rendered mephitic by one kind of contamination, and sulphurous by another, and inflammable by a third; it might even be absorbed in, and so add to the weight of, a porous solid, as water is absorbable by sand; but still air was ever indisputably air, essentially alike and unalterable in its mechanical and chemical oneness. This familiar conception had to be overcome, and the utterly strange notion of the largely aerial constitution of solid matter to be established in its stead, by the early pneumatic chemists, Black, and Cavendish, and Bergmann, before the deficiencies rather than positive errors of the phlogistic theory could be perceived.

But long ere the foundation of modern chemistry had thus been laid, in 1756, by Black's discovery of fixed air or carbonic acid as a constituent of mild alkalis and limestone, those old German doctors, Becher and Stahl, though ignorant of the nature of air and neglectful of the import of gravity, had yet found something to say about

the chemistry of combustion worthy of being defended a century afterwards by men like Priestley and Cavendish,—worthy, it is believed, of being recognized nearly two centuries afterwards as the expression of a fundamental doctrine in chemical and cosmical philosophy. They pointed out, for example, that the different and seemingly unlike processes of burning, smouldering, calcining, rusting, and decaying, by which combustible is changed into incombustible matter, have a community of character; that combustible bodies possess in common a power or energy capable of being elicited and used, whereas incombustible bodies are devoid of any such energy or power; and lastly, that the energy pertaining to combustible bodies is the same in all of them, and capable of being transferred from the combustible body which has it to an incombustible body which has it not, rendering the body that was energetic and combustible inert and incombustible, and the body that was inert and incombustible energetic and combustible—and further rendering some particular body combustible over and over again. That this is a fair representation of the views held by phlogistic chemists is readily recognizable by a study of chemical works written before the outbreak of the antiphlogistic revolution. After Lavoisier's challenge, the advocates of phlogiston, striving to make it account for a novel order of facts with which it had little or nothing to do, were driven to the most incongruous of positions; for while Priestley wrote of inert nitrogen as phlogisticated air, Kirwan and others regarded inflammable hydrogen as being phlogiston itself in the isolated state. Very different is the view of phlogiston to be gathered from the writings of Dr. Watson, for example, who was appointed Professor of Chemistry at Cambridge in 1764, became Regius Professor of Divinity in 1771, and Bishop of Llandaff in 1782. This cultivated divine, indifferent it is true to the novel questions by which in less placid regions men's minds were so deeply stirred, amused the leisure of his dignified University life by writing scholarly accounts of the chemistry it had formerly been his province to teach; and in the first volume of his well-known 'Chemical Essays,' published in 1781, the following excellent account of phlogiston is to be found:—

"Notwithstanding all that perhaps can be said upon this subject, I am sensible the reader will still be ready to ask, *What is phlogiston?* You do not surely expect that chemistry should be able to present you with a handful of phlogiston, separated from an inflammable body; you may just as reasonably demand a handful of magnetism, gravity, or electricity to be extracted from a magnetic, weighty, or electric body. There are powers in nature which cannot otherwise become the objects of sense, than by the effects they produce; and of this kind is phlogiston. But the following experiments will tend to render this perplexed subject somewhat more clear.

"If you take a piece of *sulphur* and set it on fire, it will burn entirely away, without leaving any ashes or yielding any soot. During the burning of the sulphur, a copious vapour, powerfully affect-

ing the organs of sight and smell, is dispersed. Means have been invented for collecting this vapour, and it is found to be a very strong acid. The acid thus procured from the burning of sulphur, is incapable of being either burned by itself, or of contributing towards the support of fire in other bodies: the sulphur, from which it was procured was capable of both: there is a remarkable difference, then, between the acid procured from the sulphur, and the sulphur itself. The acid cannot be the only constituent part of sulphur; it is evident that *something* else must have entered into its composition, by which it was rendered capable of combustion. This something is, from its most remarkable property, that of rendering a body combustible, properly enough denominated the food of fire, the *inflammable principle, the phlogiston*. . . . . This inflammable principle, or phlogiston, is not one thing in animals, another in vegetables, another in minerals; it is absolutely the same in them all. . . . . This identity of phlogiston may be proved from a variety of decisive experiments; I will select a few, which may at the same time confirm what has been advanced concerning the constituent parts of sulphur.

"From the analysis or decomposition of sulphur effected by burning, we have concluded that the constituent parts of sulphur are two—an *acid* which may be collected, and an *inflammable principle* which is dispersed. If the reader has yet acquired any real taste for chemical truths, he will wish to see this analysis confirmed by synthesis; that is, in common language, he will wish to see sulphur actually made by combining its acid with an inflammable principle. It seldom happens that chemists can reproduce the original bodies, though they combine together all the principles into which they have analyzed them; . . . . in the instance, however, before us, the reproduction of the original substance will be found complete.

"As the inflammable principle cannot be obtained in a palpable form separate from all other bodies, the only method by which we can attempt to unite it with the acid of sulphur must be by presenting to that acid some substance in which it is contained. Charcoal is such a substance; and by distilling powdered charcoal and the acid of sulphur together, we can procure a true yellow sulphur, in nowise to be distinguished from common sulphur. This sulphur is formed from the union of the acid with the phlogiston of the charcoal; and the charcoal may by this means be so entirely robbed of its phlogiston, that it will be reduced to ashes, as if it had been burned. . . . .

"I will in this place, by way of further illustration of the term phlogiston, add a word or two concerning the necessity of its union with a metallic earth, in order to constitute a metal. Lead, it has been observed, when melted in a strong fire, burns away like rotten wood; all its properties as a metal are destroyed, and it is reduced to ashes. If you expose the ashes of lead to a strong fire, they will melt; but the melted substance will not be a *metal*, it will be a yellow or orange-coloured *glass*. If you pound the glass, and mix it with charcoal dust, or if you mix the ashes of the lead with charcoal dust, and expose

either mixture to a melting heat, you will obtain, not a *glass*, but a *metal*, in weight, colour, consistency, and every other property the same as lead. The ashes of lead melted *without* charcoal become *glass;* the ashes of lead melted *with* charcoal become a *metal*. The charcoal, then, must have communicated *something* to the ashes of lead, by which they are changed from a glass to a metal. Charcoal consists of but two things—of ashes and of phlogiston; the ashes of charcoal, though united with the ashes of lead, would only produce glass; it must therefore be the other constituent part of charcoal, or phlogiston, which is communicated to the ashes of lead, and by an union with which the ashes are restored to their metallic form. The ashes of lead can never be restored to their metallic form without their being united with *some* matter containing phlogiston, and they may be reduced to their metallic form by being united with *any* substance containing phlogiston in a proper state, whether that substance be derived from the animal, vegetable, or mineral kingdom; and thence we conclude not only that phlogiston is a necessary part of a metal, but that phlogiston has an identity belonging to it, from whatever substance in nature it be extracted. And this assertion still becomes more general, if we may believe that metallic ashes have been reduced to their metallic form, both by the solar rays and the electrical fire."

The foregoing account by Dr. Watson is almost a translation from Stahl's '*Zymotechnica Fundamentalis, simulque experimentum novum sulphur verum arte producendi,*' in which he establishes what may be called the permanency of chemical substance—that metallic lead is reproducible from the ashes of lead, *sulphur verum* from the acid of sulphur. And, whether or not taking note of the oxidations and deoxidations effected, how little differently, even at the present day, would the actions referred to be described and explained. Is it not our habit to say that charcoal and sulphur and lead are bodies possessing potential chemical energy, that is phlogiston; that in the act of burning, their energy which was potential becomes kinetic or dynamical, and is dissipated in the form of light and heat; that the products of their burning (including the gaseous product now known to be furnished by the burning of charcoal) are substances devoid of chemical energy, that is, of phlogiston; that when the acid substance furnished by burning sulphur is heated with charcoal, some energy of the unburnt charcoal is transferred to the burnt sulphur, just as some energy of a raised weight may be transferred to a fallen one, whereby the burnt sulphur is unburnt, provided with energy, and enabled to burn again, and the fallen weight is lifted up, provided with energy, and enabled to fall again; that the potential chemical energy of metallic lead did not originate in the lead, but is energy or phlogiston transferred thereto from the charcoal by which it was smelted; and lastly, that the chemical energy of the charcoal itself, its capability of burning, its power of doing work, in one word its phlogiston, is merely a portion of energy appropriated directly from the solar rays?

If this be a correct interpretation of the phlogistic doctrine, it is

evident that the Stahlians, though ignorant of much that has since become known, were nevertheless cognisant of much that became afterwards forgotten. For most of what has since become known, mankind are indebted to the surpassing genius of Lavoisier; but the truth which he established, alike with that which he subverted, is now recognizable as a partial truth only; and the merit of his generalization is now perceived to consist in its addition to—its demerit to consist in its supersession of—the not less grand generalization established by his scarcely remembered predecessors. This being so, the relationship to one another of the Stahlian and Lavoisierian theories of combustion furnishes an apt illustration of the general truth set forth by a great modern writer, that "in the human mind, one-sidedness has always been the rule, and many-sidedness the exception. Hence, even in revolutions of opinion, one part of the truth usually sets while another rises. Even progress, which ought to superadd, for the most part only substitutes one partial and incomplete truth for another; improvement consisting chiefly in this, that the new fragment of truth is more wanted, more adapted to the needs of the time, than that which it displaces."

The partial truth contributed by Lavoisier was indeed more wanted, more adapted to the needs of the time, than the partial truth which it displaced. To him chemists are indebted for their present conception of material *elements;* and especially for their knowledge of the part played by the air in the phenomena of combustion, whereby oxygenated *compounds* are produced. The phlogistians, indeed, were not unaware of the necessity of air to combustion, but being ignorant of the nature of air were necessarily ignorant of the function which it fulfilled. To burn and to throw off phlogiston being with them synonymous expressions, the air was conceived to act by somehow or other enabling the combustible to throw its phlogiston off; and a current of air was conceived to promote combustion by enabling the combustible to throw its phlogiston off more easily. Moreover, contact of air was not essential to combustion, provided there was present instead some substance, such as nitre, which equally with, or even more effectively than air, could enable the combustible to discharge itself of phlogiston. But while the phlogistians, on the one hand, were unaware that the burnt product differed from the original combustible otherwise than as ice differs from water, by loss of energy; Lavoisier, on the other hand, disregarded the notion of energy, and showed that the burnt product included not only the stuff of the combustible, but also the stuff of the oxygen it had absorbed in the burning. But, as well observed by Dr. Crum-Brown, we now know "that no compound contains the substances from which it was produced, but that it contains them *minus* something. We now know what this something is, and can give it the more appropriate name of potential energy; but there can be no doubt that this is what the chemists of the seventeenth century meant when they spoke of phlogiston."

Accordingly, the phlogistic and antiphlogistic views are in reality complementary and not, as suggested by their names and usually maintained, antagonistic to one another. It has been said, for example, that according to Stahl, the product of combustion is simple, and the combustible a compound of the product with imaginary phlogiston—which is false; whereas, according to Lavoisier, the combustible is simple, and the product a compound of the combustible with actual oxygen—which is true. But in this case, as in so many others, everything turns upon the use of the same word in a different sense at different periods of time. When Lavoisier spoke of red lead as being metallic lead combined with oxygen, he meant that the matter or stuff of the red lead consisted of the matter or stuff of lead *plus* the matter or stuff of oxygen. But when the Stahlians spoke of metallic lead being burnt lead combined with phlogiston, they had the same sort of idea of combination in this instance, as others have expressed by saying that the weight of a body is compounded of its matter and its gravity; or that steam is a compound of water and heat; or, to use a yet more Lavoisierian expression, that oxygen gas itself is a compound of the basis of oxygen with caloric. It is not, then, that the one statement, Stahlian or Lavoisierian, is false and the other true, but that both of them are distorted, because incomplete. Chemists nowadays are both Stahlian and Lavoisierian in their notions; or have regard both to energy and matter. But Lavoisierian ideas still interfere very little with our use of the Stahlian language. While we acknowledge that in the act of burning the combustible and the oxygen take equal part, just as in the act of falling the weight and the earth take equal part, yet in our common language we alike disregard the abundant atmosphere and abundant earth as being necessarily understood, and speak only of the energy of the combustible and of the weight, which burn and fall respectively. Whatever may be the fault of language, however, chemists do not omit to superpose the Lavoisierian on the Stahlian notion. They recognize fully that it is by the union of the combustible with oxygen that phlogiston is dissipated in the form of heat; and further, that phlogiston can only be restored to the burnt combustible on condition of separating the combustible from the oxygen with which it has united; just as energy of position can only be restored to a fallen weight on condition of separating it to a distance from the surface on which it has fallen.

That Stahl and his followers regarded phlogiston as a material substance, if they did so regard it, should interfere no more with our recognition of the merit due to their doctrine, than the circumstance of Black and Lavoisier regarding caloric as a material substance, if they did so regard it, should interfere with our recognition of the merit due to the doctrine of latent heat. But though defining phlogiston as the principle or matter of fire, it is not at all clear that the phlogistians considered this matter of fire as constituting a real body or ponderable substance; but rather that they thought and spoke of it as many philosophers nowadays think and speak of the electric fluid

and luminiferous ether. The nondescript character, properly ascribable to phlogiston, is indicated by the following quotation taken from Macquer's 'Elemens de Chymie Theorique:' 1749. It must not, of course, be forgotten that the popular impression as to phlogiston having been conceived by its advocates as a material substance having a negative weight or levity, is erroneous; and is based on an innovation that was introduced during the struggling decadence of the phlogistic theory, and advocated more particularly by Lavoisier's subsequent colleague, Guyton de Morveau, in his 'Dissertation sur le Phlogistique, considéré comme Corps grave, et par rapport aux changemens de pesanteur qu'il produit dans les corps auxquels il est uni:' 1762. Macquer writes as follows:—

"La matière du soleil, ou de la lumière, le phlogistique, le feu, le soufre principe, la matière inflammable, sont tous les noms par lesquels on a coutume de désigner l'élément du Feu. Mais il paroit qu'on n'a pas fait une distinction asséz exacte . . . . du nom qu'il mérite véritablement lorsqu'il entre effectivement comme principe dans la composition d'un corps, ou bien lorsqu'il est seul et dans son état naturel. Si on l'envisage sous cette dernière vue, le nom de Feu, de matière du soleil, de la lumière et de la chaleur, lui convient particulièrement. Pour lors, c'est une substance que l'on peut considérer comme composée de particules infiniment petites, qui sont agitées par un mouvement très rapide et continuel, par conséquent essentiellement fluide. Cette substance, dont le soleil est comme le réservoir général, s'en émane perpétuellement, et est répandue universellement dans tous les corps que nous connoissons; mais non pas comme principe ou essentielle à leur mixtion, puisqu'on peut les en priver, du moins en grande partie, sans qu'ils souffrent pour cela la moindre décomposition. . . . . Cependant les phénomènes que présentent les matières inflammables lorsqu'elles brulent, nous indiquent qu'elles contiennent réellement la matière du Feu comme un de leurs principes. . . . . Examinons donc les propriétés de ce feu fixé, et devenu principe des corps. C'est lui auquel nous donnerons particulièrement le nom de matière inflammable, du soufre principe, ou de Phlogistique, pour le distinguer du Feu pur."

Again, much the same thing is to be found in Baumé's 'Manuel de Chymie:' 1765; as for example:—

"Nous considérons le feu sous deux états différens. Lorsqu'il est pur, isolé, et qu'il ne fait partie d'aucun composé . . . . Lorsqu'il est combiné avec d'autres substances, et qu'il fait un des principes constituans des corps composés . . . . On n'est pas certain si le feu est pesant. Il y a des expériences pour et contre. . . . .

"Pendant la combustion des substances, le feu combiné se reduit en feu élémentaire, et se dissipe à mesure. Le célèbre Boerhaave n'est cependant pas de ce sentiment; il dit que si cela étoit, la quantité de feu élémentaire devroit augmenter à l'infini dans la nature. . . . . Mais il est facile de répondre à cette objection, en disant comme on est en droit de le présumer, que le feu élémentaire, dégagé des corps,

se combine à mesure avec d'autres substances, et qu'il perd toutes ses propriétés de feu libre, en devenant principe constituent des corps, dans la composition desquels il entre. . . . . Le principe dont nous entendons parler ici, est celui que Stahl a nommé *phlogistique*."

In interpreting the above and other phlogistic writings by the light of modern doctrine, it is not meant to attribute to their several authors the precise notion of energy that now prevails. It is contended only that the phlogistians had, in their time, possession of a real truth in nature which, altogether lost sight of in the intermediate period, has since crystallized out in a definite form. "I trust," said Beccher, "that I have got hold of my pitcher by the right handle." And what he and his followers got hold of and retained so tenaciously, though it may be shiftingly and ignorantly, we now hold to knowingly, definitely, and quantitatively, as part and parcel of the grandest generalization in science that has ever yet been established.

[W. O.]

ANNUAL MEETING,

Monday, May 1, 1871.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1870 was read and adopted.

Eighty-one new Members were elected in 1870.

Sixty-three Lectures and Nineteen Evening Discourses were delivered during the year 1870.

The Books and Pamphlets presented in 1870 amounted to 118 volumes, making, with those purchased by the Managers, a total of 307 volumes added to the Library in the year, exclusive of periodicals.

Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers and Visitors, and to the Professors, for their services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:—

President—Sir Henry Holland, Bart. M.D. D.C.L. F.R.S.
Treasurer—William Spottiswoode, Esq. M.A. F.R.S.
Secretary—Henry Bence Jones, M.A. M.D. D.C.L. F.R.S.

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Arthur John Edward Russell, Esq. M.P.
Basil Woodd Smith, Esq.
Michael Wills, Esq.

WEEKLY EVENING MEETING,

Friday, May 5, 1871.

WILLIAM SPOTTISWOODE, Esq. M.A. & Treasurer R.S. & R.I. Vice-President, in the Chair.

W. R. S. RALSTON, Esq. M.A.

On Russian Folk-lore.

THE early history of all the Slavonic nations is shrouded in no slight obscurity, and over that of the Russians the darkness has gathered so densely that even their claim to be considered Slavonians has been contested. The statement that "if you scratch a Russian you will find a Tartar" has been amplified into a theory; and certain writers protest that the Russian people is nothing more than a collection of Finnish, Turkish, and Mongol hordes who have accepted a form of speech borrowed from their Slavonic neighbours. On this question the study of Russian Folk-lore throws a considerable amount of light, and that of a welcome kind, for by its aid it seems possible to recognize, in the ancestors of the great nation which now peoples so large a portion of Europe, men of a like race with ourselves, linked with our forefathers, not only by similar linguistic forms, but also by common religious ideas.

How and when the Slavonians entered Europe are questions which have not yet been answered with precision, but they seem to have settled at an early period along the Danube, and thence to have been obliged to migrate towards the North and East; and from those who pushed farthest towards the East the present Russians are supposed to

be descended. The mythological system of these Eastern Slavonians appears to have been of a simple nature, and never to have developed itself so fully as that of their Western brethren. They seem to have personified and adored the chief forces of Nature, and to have paid at least a certain amount of worship to the souls of the dead. Their principal deity, it is said, was originally Svarog, the Celestial Firmament, from whom sprang Ogon, Fire (the Vedic Agni), Dazhbog, the Day-God or the Sun, and Perun, the Thunder-God,—the Perkunas of the Lithuanians, whose name some philologists derive from Indra's epithet of Parjanya. They erected no temples, and they recognized no priestly caste, the elders in each family group presiding over the sacrificial rites, which were performed under the shade of trees or beside running waters.

From the reverence they paid to their dead arose their belief in a race of supernatural beings occupying a middle place between gods and men. In common with all the other Aryan races the Slavonians believed that the disembodied soul had to enter on a long pilgrimage, crossing a wide sea or climbing a steep hill. In order to help it on its way, coins were thrown into the grave together with the body it had tenanted, and also parings of nails or the claws of wild beasts—the money to pay for its passage, the claws to assist it to climb. Small ladders, also, were provided, in order that the soul might more quickly mount to heaven—a custom which has come down to the present day, the Russian peasants in many places baking in honour of their dead little ladders of dough, or pies marked with bars in imitation of a ladder's rungs. But it was not supposed that the souls of the dead were utterly cut off from the world of the living; on the contrary, it was held that many of them haunted the spots where they had been accustomed to live, and became for their relatives and descendants the source of weal or woe according as they were well treated or neglected. From these notions, modified by some of those less elevated ideas about the unseen world which the Aryan races appear to have adopted from the aboriginal inhabitants of the various countries in which they settled, probably sprang the belief in the different forms with which the fancy of the Russian peasant still peoples the woods, the waters, and the fields.

Among these the most important is the Domovoi, the Lar Familiaris, the "lubber fiend," or Hobgoblin of Russia. He is supposed to be the spirit of the original founder of the family, and so he is affectionately styled "Grandfather," and is treated with great respect. By day he lives behind the stove, but at night he comes out and assists in the work of the house, or watches over the live stock. He sympathizes intensely with the family, rejoicing when its members are merry, and grieving when they are sad. If they move into another house they take care to invite him to accompany them, performing certain rites in order to ensure his good-will towards his new abode.

The spirits of drowned women, and especially of those who drown themselves, and also those of unbaptized children, are supposed to

join the ranks of the Rusalkas, the beings who haunt the waters. They appear for the most part in the shape of beauteous maidens, whose slender forms are clothed in flowing white raiments, and over whose shoulders loosely fall masses of grass-green hair. They answer to the Lithuanian Undines and the Servian Vilas, and in many respects to our own Fairies, like whom they love to dance and sing by night beneath the moon, marking the grass with dark circles where they have moved; and they are given to sailing in egg-shells, when they find them unbroken; and they are troubled with doubts as to their future state. The little ones among them, moreover, may be saved during the space of seven years after death, if anyone who meets them will baptize them in due form. Like mermaids, the Rusalkas are constantly employed in combing their long tresses, and like the German water-spirits, they strive to entice unwary admirers into their abodes, and there they tickle them to death.

A less poetic inhabitant of the waters is the Vodyany, or male water-sprite, a bloated, red-faced, watery Silenus, who often works mischief for fishermen if they do not propitiate him by due offerings. And corresponding with him is the Lyeshy, a silvan demigod, satyr, or faun, who haunts the forest, and whose voice may be heard in the echo, and his laughter in the creaking of boughs. Since the introduction of Christianity into Russia, all these supernatural beings have sufferod considerably in reputation; one legend even attributing to them all a simply demoniacal origin, and affirming that they are that part of Satan's host which fell upon earth after having been expelled from heaven—an idea which is current in many other countries, as, for instance, in Ireland about the fairies.

Besides these divine and semi-divine, or semi-diabolical beings, there are certain supernatural personages who figure to a great extent in the stories current among the Russian peasants. Of these one of the most important is the Yagà-Baba, who answers to the Giantess, Ogress, or female Troll of Western fairy tales. She is a hideous old woman, tall and bony. She lives in a solitary hut, which turns on a pivot in obedience to a word of command, or in a larger building surrounded by a fence made of dead men's bones. As she lies indoors she stretches from one side of her chamber to the other, and her long iron nose goes right through the ceiling. When she leaves her dwelling she drives through the forest in an iron mortar, propelling it with the pestle, and sweeping away her traces with a broom. She is the type of all that is ferocious and destructive, and the mythologists recognize in her an impersonification of the storm.

Another remarkable character is Koshchei the Deathless, a demon who carries off queens and princesses from their homes, and keeps them imprisoned within his palace walls until the hero comes who is destined to set them free. In order to kill him, it is necessary to find out where his death is hidden, the result of the discovery being the same as in the Norse story of the giant who had no heart in his body, or in the Deccan tale of "Punchkin." The name of Koshchei is derived

from *Kost*, a bone, and his character is that of an ossifier or petrifier. The Russian mythologists recognize in him a type of winter, which makes all things hard and cold, and which strives to keep from the light of day the fair maiden the spring.

But the figure under which is usually personified the evil spirit which wars against sunlight and fair weather is that of the Snake. In the Russian stories it is generally a many headed, winged, fire-breathing snake, with attributes partly human and partly ophidian, who steals the beauteous maiden, or puts to death the adventurous hero, to be himself ultimately slain by some generally youthful and originally despised hero. The Snake, Koshchei, and the Yagà-Baba are all closely related to each other, seeming, indeed, to be merely different embodiments of the same spirit of darkness—of that evil principle which the old Slavonians recognized as a deity under the title of Chernobog, the Black God; a name which is in itself a sufficient proof of his supposed opposition to the spirit of light and beneficence, Byelbog, the White God.

Such are the principal mythological beings who figure in the folk-lore of the Russian peasants. That lore itself may be divided into three chief branches—the songs which the people sing, the stories which they tell, and the "metrical romances" which are recited to them by professional minstrels. These have all been so sedulously collected and edited of late years, that the books which have been devoted to them would of themselves form no small library. Many special works have also been composed on the subject of the Proverbs, the Incantations, and the other relics of the popular wisdom or superstition of old times which have come down to the present day, and which abound in Russia to an extent scarcely to be equalled in any other European country. The collection of Proverbs, for instance, made by Mr. Dahl, the author of by far the best Russian Dictionary, contains no less than 30,000 specimens, and the Zagovors, or Runes, to which special volumes have been devoted, are as numerous as they are complete.

But the most interesting among these relics of old times are the mythic and ritual songs which the heathen Slavonians used to sing at their festivals, scraps of which have been retained in the memory of the peasants, in spite of the opposition or the transforming influence of the Church. These fragments of fossilized poetry are of the greatest value as trustworthy illustrations of a very doubtful page of history, and their evidence often lends material aid in strengthening a feeble link in a chain of argument. They are now fast disappearing before the influences of civilization; but fortunately the peasants in the remoter districts of the country have preserved them until recent times with a care and conscientiousness for which all archæologists owe them a large debt of gratitude.

Each season of the year has its own songs, and their singers are careful not to mix up together what ought to be kept separate. From times immemorial the period following the winter solstice has been set

apart as a time of rejoicing for the birth of a New Year. According to a popular legend the sun—generally a female being in Slavonian mythology—then arrays herself in holiday attire, and urges her car upon the summer track. A festival, known as that of Kolyada, is held in honour of the occasion, and songs are sung, to which the name of *Kolyadki* is given, during the interval between Christmas Eve and the Epiphany. Some of them are cosmogonic, others are ritual, but all of them are of an evidently pagan origin, though Christian names have in many cases been given to the heathen divinities who are mentioned in them. The Kolyada festival is also the chief time for the *Gadaniya*, or Guesses, on which in olden times great stress was laid by all classes of the community, but which have now passed almost entirely into the hands of girls who are desirous of gaining information about their destined husbands.

In the *Kolyadki* sung during this season of the year, mention is made of a goddess called *Kolyada*, but she seems to have been merely a popular embodiment of the festival. The songs called *Ovsenevuiya*, however, seem really to have been consecrated to a deity named Ovsen—a "good youth," who makes the way clear for the New Year, and brings with him from Paradise rich gifts of fruitfulness, and whom some of his attributes seem to link with Freyr, the Teutonic Sun-God.

The death of winter and the arrival of the spring are typified in Russia by various observances, and a great number of songs called *Vesenniya* are then sung in honour of the coming of *Vesna*, the vernal season. In them constant allusions are made to Lado and Lada, the god and goddess of love and fertility. Easter brings with it a number of songs which in olden times were consecrated to some heathen festival in honour of the revival of the season of light and warmth, which during the cold winter-tide has laid torpid and apparently dead, the Paschal week having received from some such association of ideas the epithet *Svyetlaya* or bright. With the first week after Easter commences the festival of the *Krasnaya Gorka*, or "red hill," the epithet referring, like the redness of the well-known Easter eggs, to the brightness of the spring, and the name of "hill" having been given to it because it was originally held on some high place. Its chief feature is the *Khorovod*—the circling dance attended by choral song which has always been a marked feature of Slavonian festivities. This is also the season of the *Radunitsa* feast, held in honour of the dead, who are supposed at this time of the year to awake from their winter sleep, and to rejoice (radovat') in unison with their living descendants, who go out to their graves and there indulge in banquets which still retain something of their old sacrificial character.

The 23rd of April is consecrated to St. George, and to it are devoted a number of songs called after his name. From the nature of their contents it is evident that the Christian saint has merely taken the place of an old heathen deity, for it is not a slayer of dragons who appears in them, but a patron of farmers and herdsmen,

who preserves cattle from harm, and on whose day, therefore, the flocks and herds are for the first time after the winter sent out into the open fields. On the Thursday of the seventh week after Easter is held the feast called *Semik* (from Sem = Seven). In heathen times a number of rites used to be performed during May, in honour of the goddess of the spring; a portion of these rites were eventually transferred with altered names to the festivals of Ascension Day and Whitsuntide. In that way many of them have become attached to the Semik holiday, when the young people go into the woods and there cut down a birch tree and dress it in female attire, or adorn it with ribbons (much as we used to deck on May poles), singing the while a special set of songs in which some remains of tree worship make themselves clearly apparent.

Midsummer brings with it certain festivals evidently of the same origin as those of which some traces are still found in so many countries of Western Europe, and on them the Russian peasants sing the class of songs called *Kupalskiya*, the epithet referring to a mythological being called Kupalo, whose image is in some parts of Russia burnt or thrown into a stream on St. John's night. The Midsummer bonfires, and the various ideas connected with the eve and the day of St. John, form strong links in the chain of evidence which binds together the modern Russians with the rest of the members of the Aryan family.

The 29th of July forms, in the popular Kalendar, the first autumnal festival. In olden times the day was consecrated to Perun, the Thunder-God. Since the introduction of Christianity it has been transferred to Ilya the Thunderer, as the Servians call the Prophet Elijah. The number of legends, strongly marked by heathen characteristics, referring to the saint who has usurped the place of the pagan deity, is very great; but, except among the Bulgarians, there are no special Slavonic songs devoted to his festival. At the time of the harvest many old customs are observed; some of which are now said to be in honour of Elijah or of St. Nicholas, but which seem to have originally referred to Perun, who at that season of the year was honoured under the name of Volos.

During the interval between harvest-time and Christmas-tide many other popular festivals occur, several of which are marked by strange customs, such as the burying of insects in vegetable coffins on the first of September, and the expulsion of cockroaches on the eve of St. Philip's Fast; but the poetry which is devoted to them is not for the most part of any great mythological or historical interest. The songs of modern date, which are generally distinguished from those of old times by being rhymed, are in themselves by no means undeserving of study, but they are of little worth as compared with those invaluable records of the past, their elder brethren.

Of the songs of the Russian people various collections have been made at different times, but it is only recently that their stories have been gathered together and edited in any scientific manner. This

work has lately been performed by A. N. Afanasief, of Moscow, with as much patience and conscientiousness as Mr. J. F. Campbell has bestowed upon his edition of the 'Tales of the West Highlands,' the genuineness of the stories being guaranteed by the fact that the names of their narrators or transcribers are almost always set forth. The work, which is divided into eight parts, extends to as many as 2394 pages, and contains 332 distinct stories, of some of which as many as five variants are given. If it were merely on account of the freshness and simplicity of their style, and of their, in many cases, supplying features which our Western versions of old Eastern tales have lost, these Skazki (so called from Skazat' = to say, or tell) would be of great interest; but they derive a special value from the light they throw upon the question of the origin of European romance, and especially upon the theory upheld by Benfey in his erudite introduction to the *Pantschatantra*, that our fairy tales, fables, and the like, were first introduced into Europe during the Middle Ages through the agency of the Mahomedans and Mongols. For almost all the leading tales current in Western Europe it is possible to find a Russian counterpart, and in many cases the Slavonic rendering will be found to offer sense for what in the Teutonic or Celtic version of some Oriental expression has become mere nonsense.

The origin of the Russian popular fictions has lately excited much controversy in Russia, Mr. Stasof, of the Imperial Library of St. Petersburg, having broached a theory that they are not only of Oriental parentage—which is undoubtedly the case—but that they have been introduced into the country at a comparatively recent date, by the Turkish and Mongolian tribes of Central Asia. The Russians possess a great store of what they call *Builinui*, a word for which perhaps the best rendering is, "metrical romances," unrhymed epic poems, varying from a hundred to a thousand lines in length, in which are sung the doughty deeds of various national heroes, the greater part of whom are supposed to have been attached to the person of Vladimir, Prince of Kief. There are many *builinui* about Novgorod, and there is also a "Moscow Cycle"; but by far the most important are included in what is known as the "Vladimir Cycle," which is for Russia what the Carlovingian and Arthurian Cycles of romance are for France and Britain. Into the nature of these poems, in which are sung the adventures in love and in war of Ilya Muromets, the most popular of the old heroes, of Dobruinya Nikitich, the Prince's nephew, the Roland of the Vladimir Cycle, and of Alyësha Popovich, the Priest's son, it is impossible to enter at present. It must suffice to state that in them Slavophile commentators are inclined to see a picture of Old Russia as it used to be before the Tartar invasion, while Mr. Stasof recognizes in them merely imitations, if not actual translations of such Tartar poems and tales as have been collected and translated (into German) by Professor Schiefner in his 'Heldensagen der Minussinischen Tataren,' and by Professor Radloff in his admirable 'Proben der Volkslitteratur der Türkischen Stämme Süd-Siberiens.' The

real truth seems to be, that while almost all the fragments of mythic and ritual song preserved among the Russian people are probably relics of a great mass of poetry common to the Slavonic tribes at the time when they first arrived in Europe, at least a great part of the tales and "metrical romances" now current among the Russians have most likely come to them at various times from different quarters, traces of Scandinavian, of Finnish, of Mongol, and of other influences being apparent in the forms of what seem to have originally been Indian and Persian fictions. But whatever else may be said of it, this much seems to be clear, that the Russian folk-lore forms a part of the rich mythological inheritance held in common by the various branches of the great Aryan family.

[W. R. S. R.]

GENERAL MONTHLY MEETING,

Monday, May 8, 1871.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

The following Vice-Presidents were nominated for the ensuing year:—

The Duke of Northumberland.
The Lord Lindsay.
Sir Frederick Pollock, Bart.
William Spottiswoode, Esq. the Treasurer.

William S. Burton, Esq.
Arthur Samuel Hobson, Esq. F.C.S.
Richard Liebreich, Esq. M.R.C.S.
Abraham De Mattos Mocatta, Esq.
Edward Stanhope Pearson, Esq.

were *elected* Members of the Royal Institution.

JOHN TYNDALL, Esq. LL.D. F.R.S. was re-elected Professor of Natural Philosophy.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Accademia Pontificia de' Nuovi Lincei, Roma—Atti: anno XXII. 1868-9. 4to.
Actuaries, Institute of—Journal, No. 73. 8vo. 1871.
Antiquaries, Society of—Archæologia, Vol. XL. Part 2. 4to. 1871.
Asiatic Society of Bengal—Proceedings, 1870, Dec. 1871, Jan. 8vo.

Astronomical Society, Royal—Proceedings, Vol. XXXI. No. 5. 1871.
Baxter, R. Dudley, Esq. M.R.I. (the Author)—National Income. The United Kingdom. 8vo. 1868.
The Taxation of the United Kingdom. 8vo. 1869.
National Debts. 8vo. 1871.
British Architects, Royal Institute of—Sessional Papers, 1870-71, Nos. 6, 7, 8. 4to.
British Museum Trustees—List of Books of Reference in the Reading-room of the British Museum. 2nd ed. 8vo. 1871.
Supplement to the Catalogue of Seals and Whales. 8vo. 1871.
Chemical Society—Journal for April, 1871. 8vo.
Clark, Latimer, and Sabine, Robert, Esqrs. (the Authors)—Electrical Tables and Formulæ. 16mo. 1871.
Davies, Lieut. A. M., R.A. M.R.I. (the Author)—Meteoric Theory of Saturn's Rings; and of the Sun. 8vo. 1871.
Editors—Academy for Feb. 1871. 4to.
American Journal of Science, March, 1871. 8vo.
Artizan for April, 1871. 4to.
Athenæum for April, 1871. 4to.
Chemical News for April, 1871. 4to.
English Mechanic for April, 1871.
Engineer for April, 1871. fol.
Food Journal for April. 8vo. 1871.
Horological Journal for April, 1871. 8vo.
Journal of Gas-Lighting for April, 1871. 4to.
Mechanics' Magazine for April, 1871. 8vo.
Nature for April, 1871. 4to.
Pharmaceutical Journal for April, 1871. 8vo.
Photographic News for April, 1871. 4to.
Scientific Review for April, 1871. fol.
Franklin Institute—Journal, Nos. 540, 541. 8vo. 1871.
Geographical Society, Royal—Proceedings, No. 15. 8vo. 1871.
Geological Institute, Imperial, Vienna—Jahrbuch, 1870. No. 4. 8vo.
Verhandlungen, 1870. No. 13. 8vo.
Geological Society—Quarterly Journal, No. 106. 8vo. 1871.
Institution of Mechanical Engineers—Proceedings, Jan. 1871. 8vo.
London Institution—Journal, No. 4. 8vo. 1871.
Meteorological Office.—Quarterly Weather Report, 1869. IV. 4to. 1870.
Meteorological Society—Proceedings, Nos. 53, 54. 8vo. 1871.
N. A. V. (the Author)—Patent Law and Practice. 16mo. 1871.
Photographic Society—Journal, No. 224. 8vo. 1871.
Preussische Akademie der Wissenschaften—Monatsberichte, Jan. Feb. Marz, 1871. 8vo.
Roberts, W. Chandler, Esq. M.R.I.—First Annual Report of the Deputy-Master of the Mint. 8vo. 1871.
Report on Brittle Gold, &c. 8vo. 1871.
Royal Society of London—Proceedings, Nos. 127. 8vo. 1871.
Squire, Peter, Esq. F.C.S. M.R.I. (the Author)—Companion to the British Pharmacopœia. 8th ed. 8vo. 1871.
Statistical Society—Journal, Vol. XXXIV. Part 1. 8vo. 1871.
Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, April, 1871. 8vo.
Tyndall, John, Esq. LL.D. F.R.S. (the Author)—Fragments of Science for Unscientific People. 8vo. 1871.
United Service Institution, Royal—Journal, Appendix. 8vo. 1871.
Zennaro, Dr. S.—Etude sur le Choléra; Constantinople, 1865. 8vo. 1870.

WEEKLY EVENING MEETING,

Friday, May 12, 1871.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

COL. W. F. DRUMMOND JERVOIS, R.E. C.B.

On the Defensive Policy of Great Britain.

DURING the year 1869, the total tonnage of British and foreign vessels entered and cleared at ports of the United Kingdom was no less than 34,910,281, of the total value of 532,475,266*l*. Of this enormous trade, a large proportion is of the last importance, not only to the prosperity, but to the very life of the country. Grain and flour to the value of thirty-seven millions sterling were imported in 1869. Whatever may happen, it is absolutely necessary that this trade should not be altogether stopped. Some millions' worth of foreign corn we must have, or we shall starve.

There is a trade of about 98 millions with the northern and the western countries of Europe; of 72½ millions with the countries bordering the Mediterranean; of 83 millions with India and the east, of which a portion also passes through the Mediterranean; of 11 millions with Africa and the Mauritius; and of 25 millions with Australasia. Turning to the westward, we have a trade of 51½ millions with the West Indies, Central and South America, and of no less than 80 millions with the United States and our Colonies in North America. . . . .

We must have swift and powerfully-armed vessels adapted to cope with the cruisers which would be launched against our trading ships, and armour-plated men-of-war to meet external attacks which might be made against foreign territories under British rule.

In connection with the necessity for the maintenance of naval squadrons abroad, it is of paramount importance to defend effectually the foreign stations at which our cruisers must be coaled and repaired. . . . .

Malta and Gibraltar for the Mediterranean,—Halifax and Bermuda for the Atlantic,—Port Royal, Jamaica, from its position with reference to the West Indies and the Gulf of Mexico,—Bombay and Aden,—Simon's Bay, at the Cape of Good Hope,—Port Louis, Mauritius,—a harbour in Ceylon,—Singapore,—Hong Kong,—and some other ports, are, in military language, the strategical bases for our foreign squadrons.

The defences of several of our important Colonial Stations are being provided for by the respective Colonies themselves. But they cannot

yet take part in their external protection, and this we acknowledge that it is our duty to afford.

One most important function the Navy has to perform is to keep open the ports of the United Kingdom for the ingress and egress of our merchant-ships. Unless the shipping of the Thames, the Mersey, the Clyde, and numerous other ports be free to come and go, it will be of little avail to protect our *foreign* possessions. The measures adopted should of course be suited to the circumstances of each locality. As regards fixed defences on shore, an idea is prevalent that earthwork batteries could easily be extemporized for the protection of our Commercial Ports, but to anyone who is aware of the nature of the appliances of a modern battery, intended for defence against attack by the powerful guns and ships of the present day, such proposals are known to be utterly delusive. Sometimes the defence of a port can be wholly provided for by shore batteries, supplemented by torpedoes; sometimes it is best to provide for it partly by shore batteries and partly by turret-vessels and gunboats, aided by torpedoes; and in other cases it may be desirable to employ floating batteries only in the defence.

As regards garrisons for the works defending our Commercial Harbours, besides the Militia, we have large bodies of Volunteer Artillery at most of our seaports. These, if trained to the service of heavy rifled guns, by a few experienced Royal Artillerymen who should be stationed at each place, and who should have charge of the works and armaments, would be thoroughly efficient for the service of the guns in the batteries. Without such batteries, these Artillerymen are like infantry without rifles, and are useless. On the other hand, if the batteries were provided, they would form a very important element in our arrangements for defence.

Whilst these would man the land batteries, the gun-boats might be manned by local seamen, under Naval Officers. A few Military Engineers would be required for the working of torpedoes, the stores for which do not deteriorate by keeping, and should always be ready on the spot. The Defence of the Commercial Ports would thus be complete. . . . .

Our maritime frontier must be placed—it is to a great extent already placed—in a condition for defence. We have what I may call naval entrenched camps on the Southern Coast of the kingdom at Portsmouth and Plymouth, at Portland, at Pembroke, at Chatham, Dover, and Cork. A large fortified harbour on the Eastern Coast is still wanted. In the late discussions in Parliament, the proposal for a harbour at Filey Bay was discussed as if it were solely required as a refuge for the mercantile marine; but such a harbour, if fortified, would be the Portland of the Eastern Coast. From these naval entrenched camps our squadrons for the Coast Defence would issue for the protection of the adjacent shores; upon them our fleet might retire in security if assailed by superior force. The fortifications of

such places protect the basins, the docks, the factories, the stores, the coaling and victualling establishments, the magazines, the anchorages for our Navy, and when properly garrisoned they render them perfectly secure against attack.

Some other harbours, such as Harwich and Newhaven, would also form valuable *points d'appui* for gun-boats and light-draught vessels, acting for the defence of the coast; and for this as well as for another reason, *viz.* that they would be good bases of operation for an enemy, they are being fortified; indeed a strong fort, which will completely protect the harbour at Newhaven, is just completed.

The Humber, which in addition to its value as a commercial port is a harbour of strategical importance, should also be strongly defended; and the several anchorages within certain limits, in which an enemy's fleet might lie, with a view to ulterior operations, should, where practicable, be denied to him, by some powerful guns placed in small secure forts.

There are several minor harbours, *viz.* Poole, Chichester, Littlehampton, Shoreham, Folkestone, Rye, Ramsgate, the Blackwater, &c., which though only tidal, would form very convenient places for an enemy to land his artillery, horses, and stores. These ought to be defended by a small strong work at each, to prevent their seizure by an enemy, and thus the operations of an invading force would be restricted to the open beach, and rendered liable to interruption from bad weather. . . . .

As regards the Regular army, it is generally considered that we should maintain a comparatively small force, complete in all its branches, and that after having received a thorough training in this army, men should be passed to a Reserve, by means of which, on occasion requiring, the available regular force may be largely increased. Some propose that the Militia shall be reorganized, and more or less connected with the regiments of the regular army. With regard to the Volunteers, every possible encouragement and assistance should be afforded to bring these forces up to such a standard of efficiency as will enable them to fulfil properly the rôle assigned to them in National Defence. . . . .

Let us consider the plan of operations which an invader would be likely to adopt. There can be no doubt but that his great object would be to march upon London, and with this view his main descent would be upon our Eastern or Southern shores. If Portsmouth and Plymouth were not fortified to landward, he might detach a corps to burn the ships and naval establishments at one of these places. The fortifications of these places, however, garrisoned by Auxiliary Forces, with a nucleus of well-trained troops, would render these great arsenals secure. The enemy might make a feint, perhaps an attack, by a corps directed upon Ireland. In any case we must leave a large body of troops there. Perhaps, instead of a descent upon Ireland, he might send a corps to the Yorkshire or Lincolnshire coast, in addition to the

main attack upon the Eastern or Southern coast. He would thus at least distract the attention of our defensive forces, and possibly this subsidiary attack might turn out to be one of great importance.

You will observe that if he succeeded in making good a landing with a corps at or somewhere near the Humber, he would be in a position to march upon the great seats of manufacture, and a successful advance against Manchester, Liverpool, Sheffield, and other places in the same quarter, would probably result in enormous requisitions being levied upon these seats of wealth and manufacturing industry.

In view of an operation of this description, the defence of the Humber, to which I have before adverted, and the establishment of a "Central Arsenal," which should probably be somewhere near Sheffield (in preference to Cannock Chase, where it has hitherto been proposed to place it), are both matters of very great importance.

At the same time that an attack or a feint was made on the North-Eastern Counties, the landing of the main invading force might be effected on the Eastern or on the Southern coast with a view of marching upon London.

Until it be clearly seen whether the attempt to the Northward was a feint or a real attack, we must keep a force there to meet it. When the line of the attack from the Southward was fully developed, we should concentrate upon it, and dispute the possession of the soil, as far as practicable; of course bringing down the force from the north if not required there. The whole of the active army, except the force in Ireland, and such portions as must be detached for the nucleus of each of the garrisons of our Naval Arsenals and other fortresses, should be brought together to oppose the advance on London.

All railway arrangements for the ready transport of troops would be made by the Military Staff, in concert with the managers of the several lines. The telegraph lines are now in the hands of the Government, and would therefore be worked by the same employés as at present.

Temporary works, on the probable or expected fields of battle, would be thrown up between London and the Coast; these would be executed by bodies of workmen, organized under the Civil Engineer Staff Corps, and under the direction of Military Engineers, by whom the plans would have been previously prepared. The number of men and tools, as well as the time required for forming each work, would have been previously calculated.

Roads would be broken up, obstacles across them created, railways destroyed, and we should probably fight a pitched battle either on the ranges of chalk hills in Kent or Surrey, near Chelmsford, or in some chosen positions elsewhere.

Fight, no doubt, we should with fury, but we might be beaten!! If so the enemy marches straight into London. Our army might retire upon the fortified entrenched camp at Portsmouth, on the one hand, or upon the fortified Central Arsenal, if we had one, on the other; but with London in the hands of the enemy, what then?

Some say even if London were occupied by a hostile army, we should still continue the struggle. Look, they say, at the case of Madrid in the Peninsular War! Look at Moscow in 1812! Look at Vienna in the wars of the beginning of this century.

But it is almost beside the question to compare these cases, or that of Berlin and other capitals, except Paris, with that of London. The fall of London would render further resistance impossible. With London in the hands of an enemy, then the seat of Government, the heart of the Empire, the centre of all commerce, the focus of our communications, our Government Factories at Woolwich for guns, gun-carriages, munitions, stores (the only Government manufacturing arsenal we possess), the great Naval Arsenal at Chatham, too (which is unfortified to landward), all fall into his hands!! There would be a collapse of commerce, of government, and of order, from which there might be no recovery.

What shall the measures of precaution be?

The Duke of Wellington, in his memorable letter of January, 1847, said, "I know of no mode of resistance, much less of protection, "from this danger except by an army in the field capable of meeting "and contending with its formidable enemy, aided by all the means "of fortifications which experience in war and science can suggest."

As regards the Army, I believe that if the regular and other forces which it is proposed to maintain, be trained, so as to be efficient for the duties they may be liable to perform, and if, to use the Duke of Wellington's words, they be "aided by all the means of fortifications "which experience in war and science can suggest," the defence of the country against invasion will be fully provided for.

Without the aid of fortifications, I submit that it will not be *possible*, unless compulsory service be resorted to, to organize and maintain an army capable of affording that complete security, to obtain which our military expenditure is incurred.

In this country, on account of its expense and the withdrawal of men from industrial pursuits, which it involves, there is great repugnance to a large standing army.

The cost of fortifications is very small when compared with that of the maintenance of regular troops. The capital cost of an addition of only 5000 men to our regular army may be stated as follows:—

	£	£
5000 men, with barracks and non-effective charges, at 60*l.* per man per annum ..	300,000	
Capitalized at 3½ per cent.		9,000,000
Productive labour of 5000 men lost by their being converted into soldiers, say 30*l.* per annum	150,000	
Capitalized at 3½ per cent.		4,500,000
Total cost of 5000 regular troops		£13,500,000

Now, an augmentation of 5000 men to our regular army is scarcely

worth mentioning as a contribution to the increase of our defensive power, whereas the cost of this addition, if applied to fortification, would, with the forces already proposed to be maintained, for ever settle the question of the invasion of England. . . . .

The expense of the necessary permanent fortifications around the metropolis would be about eight millions sterling. Add to this the cost of the Central Arsenal and of the works required for the protection of the seaports at home and abroad above referred to, and the whole expense, including armaments, would not be so great as the capital cost of 5000 regular troops. The expense of the maintenance of fortifications is but trifling, and we already have more than an abundance of forces capable of manning them.

The works for the defence of London should be permanent; and provided with plenty of bomb-proof cover and deep well-flanked ditches. They should be impregnable against assault, and the ramparts should be constructed so as to render the artillery therein secure against being silenced by the enemy's fire.

As regards the question where the works should be, there are two principles which have to be considered. One may be called the indirect, the other the direct system.

The indirect plan is, to construct at a distance of 20 or 30 miles from London, three or four fortified entrenched camps; one at Chatham (where, in any case, the works proposed by the Defence Commission for the defence of the Naval Arsenal should be carried out), one west, another north, and another south of London. The direct plan is, to construct a series of detached forts, crossing fire with each other, and from about 2000 to 3000 yards apart, according to the circumstances of the locality, all round London, at a distance of about 12 miles from its centre.

The principle on which the indirect system is advocated is, that each entrenched camp, or strategical fortress, would be capable of containing a large force which might act on the flank or rear of an enemy, and threaten his communications if he passed it; and that an invader would therefore be obliged to sit down and besiege it, or to employ a large force to mask it, before he could proceed on his march to London.

A system of purely strategical fortresses, such as we are discussing, would be inoperative unless the forces acting from them were fully equipped, thoroughly trained, and disciplined troops. Even admitting that we had sufficient forces of this description capable of taking the field, the safety of London would still be dependent on the result of a general action. . . . .

Now, the direct system of defence would absolutely cover London; and the whole adult male population, as also the enormous resources of all kinds within the line of the proposed forts, could be drawn upon for the defence at any part of the circle. A ring of about fifty works would at every point afford a strong fortified battle-field twelve miles from the centre of London. . . . .

The perimeter of the line of fortifications being more than seventy-five miles in extent, it is impossible that it could be invested. The circle of the forts round Paris is less than thirty miles, and it took a quarter of a million of troops to invest it. To invest London, if defended as proposed, would require an army of 700,000 men. The distance of the works from London would be greater than necessary to protect even the suburbs from bombardment.

Paris held out for five months, and then only gave in from want of food,—the bombardment did not hasten the surrender for one hour. If Bazaine's army had retired upon Paris instead of remaining at Metz after the battle of Woerth; if MacMahon had gone to Paris or to Orleans instead of to Sedan; or if Metz had held out for a fortnight longer than it did, the result of the late war would have been very different from what it was. Paris would have been relieved, and the Germans would have been in a most critical position. The works round London, if we *were* invaded, and the army defeated in the field, would enable us to retrieve the fortunes of England!

[W. F. D. J.]

WEEKLY EVENING MEETING,

Monday, May 19, 1871.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Professor Huxley, F.R.S.

On Bishop Berkeley and the Metaphysics of Sensation.

In his introductory remarks, Professor Huxley commended the new edition of 'Berkeley's Works, with his Life,' by Professor Fraser, as for the first time doing justice to the subtle and penetrating mind of one of the noblest and purest figures of his time, and highly esteemed by his illustrious contemporaries. Berkeley lived at a very remarkable epoch in our intellectual history (1684–1753), when the political fermentation of the preceding age was gradually working itself out; domestic peace gave men time to think; and toleration permitted great freedom of speech and writing. The great faculty of our race for physical and metaphysical inquiry consequently developed itself vigorously; and two of its most permanently influential products were—English Freethinking, and the Theory of Gravitation: results of intellectual impulses which derived their origin from Herbert, Hobbes, Bacon, and especially from Descartes. The Cartesian doubt—the maxim that assent may properly be given

to no propositions but such as are perfectly clear and distinct—became incarnate, so to speak, in Anthony Collins, Toland, Tindal, and Pierre Bayle, and reached its final term in Hume.

On the other hand, although the theory of Gravitation set aside the Cartesian vortices—yet the spirit of the 'Principes de Philosophie' attained its apotheosis when Newton demonstrated all the host of heaven to be but the elements of a vast mechanism, regulated by the same laws as those which govern the falling of a stone to the ground. There is a passage in the preface to the first edition of the 'Principia' which shows that Newton was penetrated, as completely as Descartes, with the belief that all the phenomena of nature are expressible in terms of matter and motion. . . .

But the doctrine that all the phenomena of nature are resolvable into mechanism is what people have agreed to call "materialism"; and when Locke and Collins maintained that matter may possibly be able to think, and Newton himself could compare infinite space to the sensorium of the Deity, it was not wonderful that the English philosophers should be attacked as they were by Leibnitz in the famous letter to the Princess of Wales, which gave rise to his correspondence with Clarke. . . . The phenomena of nature were held to be resolvable into the attractions and the repulsions of particles of matter; all knowledge was attained through the senses; the mind antecedent to experience was a *tabula rasa*. In other words, the character of speculative thought in England at the commencement of the eighteenth century was essentially sceptical, critical, and materialistic. . . . .

George Berkeley was destined to give the same kind of development to the idealistic side of Descartes' philosophy that the Freethinkers had given to its sceptical side and the Newtonians to its mechanical side. He said to the materialists: "You tell me that all the phenomena of nature are resolvable into matter and its affections. I assent to your statement, and now I put to you the further question, 'What is matter?' In answering this question you shall be bound by your own conditions; and I demand, in the terms of the Cartesian axiom, that in turn you give your assent only to such conclusions as are perfectly clear and obvious."

It is this great argument which is worked out in the 'Treatise concerning the Principles of Human Knowledge,' and in those 'Dialogues between Hylas and Philonous,' which rank among the most exquisite examples of English style, as well as among the subtlest of metaphysical writings; and the final conclusion of which is summed up in a passage remarkable alike for literary beauty and for calm audacity of statement.

"Some truths there are so near and obvious to the mind that a man need only open his eyes to see them. Such I take this important one to be, *viz.* that all the choir of heaven and furniture of the earth—in a word, all those bodies which compose the mighty frame of the world—have not any substance without a mind; that their being is to be perceived or known; that consequently, so long as they are not actually perceived by me, or do not exist in my mind or that of any other created spirit, they must either have no existence at all or else subsist in the mind

of some eternal spirit; it being perfectly unintelligible, and involving all the absurdity of abstraction, to attribute to any single part of them an existence independent of a spirit."—*Treatise concerning the Principles of Human Knowledge*, Part I., § 6.

Doubtless this passage sounds like the acme of metaphysical paradox, and we all know that "coxcombs vanquished Berkeley with a grin"; while common-sense folk refuted him by stamping on the ground or some such other irrelevant proceeding. But the key to all philosophy lies in the clear apprehension of Berkeley's problem—which is neither more nor less than one of the shapes of the greatest of all questions, "What are the limits of our faculties?" And it is worth any amount of trouble to comprehend the exact nature of the argument by which Berkeley arrived at his results, and to know by one's own knowledge the great truth which he discovered—that the honest and rigorous following up of the argument which leads us to materialism, inevitably carries us beyond it.

Suppose that I accidentally prick my finger with a pin. I immediately become aware of a condition of my consciousness—a feeling which I term pain. I have no doubt whatever that the feeling is in myself alone; and if anyone were to say that the pain I feel is something which inheres in the needle, as one of the qualities of the substance of the needle, we should all laugh at the absurdity of the phraseology. In fact, it is utterly impossible to conceive pain except as a state of consciousness.

Hence, so far as pain is concerned, it is sufficiently obvious that Berkeley's phraseology is strictly applicable to our power of conceiving its existence—"its being is to be perceived or known," and "so long as it is not actually perceived by me, or does not exist in my mind, or that of any other created spirit, it must either have no existence at all, or else subsist in the mind of some eternal spirit." . . . . So much for pain. And the same reasoning applies to all the other simple sensations. . . . .

It is undoubtedly true that, as Berkeley says, their "*esse* is *percipi*"—their being is to be "perceived or known." But that which perceives, or knows, is mind or spirit; and therefore that knowledge which the senses give us is, after all, a knowledge of spiritual phenomena.

All this was explicitly or implicitly admitted, and, indeed, insisted upon, by Berkeley's contemporaries, and by no one more strongly than by Locke, who terms smells, tastes, colours, sounds, and the like, "secondary qualities," and observes, with respect to these "secondary qualities," that "whatever reality we by mistake attribute to them [they] are in truth nothing in the objects themselves." . . . .

Thus far then materialists and idealists are agreed. Locke and Berkeley, and all logical thinkers who have succeeded them, are of one mind about secondary qualities—their being is to be perceived or known—their materiality is, in strictness, a spirituality.

But Locke draws a great distinction between the secondary qualities of matter, and certain others which he terms "primary qualities."

These are extension, figure, solidity, motion and rest, and number; and he is as clear that these primary qualities exist independently of the mind, as he is that the secondary qualities have no such existence. . . . . He is as thoroughgoing a realist as St. Anselm. In Berkeley, on the other hand, we have as complete a representative of the nominalists and conceptualists—an intellectual descendant of Roscellinus and of Abelard. And by a curious irony of fate, it is the nominalist who is, this time, the champion of orthodoxy, and the realist that of heresy.

Once more let us try to work out Berkeley's principles for ourselves, and inquire what foundation there is for the assertion that extension, form, solidity, and the other "primary qualities," have an existence apart from mind. And for this purpose let us recur to our experiment with the pin.

It has been seen that when the finger has been pricked with a pin, a state of consciousness arises which we call pain; and it is admitted that this pain is not a something which inheres in the pin, but a something which exists only in the mind, and has no similitude elsewhere.

But a little attention will show that this state of consciousness is accompanied by another, which can by no effort be got rid of. I not only have the feeling, but the feeling is localized. I am just as certain that the pain is in my finger, as I am that I have it at all. Nor will any effort of the imagination enable me to believe that the pain is not in my finger. And yet nothing is more certain than that it is not, and cannot be in the spot in which I feel it, nor within a couple of feet of that spot. For the skin of the finger is connected by a bundle of fine nervous fibres, which run up the whole length of the arm, with the spinal marrow and brain, and we know that the feeling of pain caused by the prick of a pin is dependent on the integrity of those fibres. If they be cut through close to the spinal cord, no pain will be felt, whatever injury is done to the finger; and if the ends which remain in connection with the cord be pricked, the pain which arises will appear to have its seat in the finger just as distinctly as before. Nay, if the whole arm be cut off, the pain which arises from pricking the nerve stump will appear to be seated in the fingers, just as if they were still connected with the body.

It is perfectly obvious, therefore, that the localization of the pain at the surface of the body is an act of the mind. It is an *extradition* of that consciousness, which has its seat in the brain, to a definite point of the body—which takes place without our volition, and may give rise to ideas which are contrary to fact. We might call this extradition of consciousness a reflex feeling, just as we speak of a movement which is excited apart from, or contrary to, our volition, as a reflex motion. Locality is no more in the pin than pain is; of the former, as of the latter, it is true that "its being is to be perceived," and that its existence apart from a thinking mind, is not conceivable.

The foregoing reasoning will be in no way affected, if, instead of

pricking the finger, the point of the pin rests gently against it, so as to give rise merely to a tactile sensation. The tactile sensation is referred outwards to the point touched, and seems to exist there. But it is certain that it is not and cannot be there really, because the brain is the sole seat of consciousness; and, further, because evidence, as strong as that in favour of the sensation being in the finger, can be brought forward in support of propositions which are manifestly absurd.

For example, the hairs and nails are utterly devoid of sensibility, as every one knows. Nevertheless, if the ends of the nails or hairs are touched, ever so lightly, we feel that they are touched, and the sensation seems to be situated in the nails or hairs. Nay more, if a walking-stick a yard long is held firmly by the handle and the other end is touched, the tactile sensation, which is a state of our own consciousness, is unhesitatingly referred to the end of the stick; and yet no one will say that it *is* there.

Let us now suppose that, instead of one pin's point resting against the end of my finger, there are two. Each of these can be known to me, as we have seen, only as a state of a thinking mind, referred outwards, or localized. But the existence of these two states, somehow or other, generates in my mind a host of new ideas, which did not make their appearance when only one state was present.

For example, I get the ideas of co-existence, of number, of distance, and of relative place or direction. But all these ideas are ideas of relations, and imply the existence of something which perceives those relations. If a tactile sensation is a state of the mind, and if the localization of that sensation is an act of the mind, how is it conceivable that a relation between two localized sensations should exist apart from the mind? It is, I confess, quite as easy for me to imagine that redness may exist apart from a visual sense, as it is to suppose that co-existence, number, and distance can have any existence apart from the mind of which they are ideas.

Thus it seems clear that the existence of some, at any rate, of Locke's primary qualities of matter, such as number and extension, apart from mind, is as utterly unthinkable as the existence of colour and sound under like circumstances.

Will the others, namely, figure, motion and rest, and solidity, withstand a similar criticism? I think not. For all these, like the foregoing, are perceptions by the mind of the relations of two or more sensations to one another. . . . .

If freedom to move in all directions is the very essence of that conception of space of three dimensions which we obtain by the sense of touch; and if that freedom to move is really another name for the feeling of unopposed effort, accompanied by that of change of place, it is surely impossible to conceive of such space as having existence apart from that which is conscious of effort.

But it may be said that we derive our conception of space of three dimensions not only from touch, but from vision; that if we do not

feel things actually outside us, at any rate we see them. And it was exactly this difficulty which presented itself to Berkeley at the outset of his speculations. He met it, with characteristic boldness, by denying that we do see things outside us; and, with no less characteristic ingenuity, by devising that "New Theory of Vision" which has met with wider acceptance than any of his views, though it has been the subject of continual controversies.

In the 'Principles of Human Knowledge,' Berkeley himself tells us how he was led to those views which he published in the 'Essay towards the New Theory of Vision.'

"It will be objected that we see things actually without, or at a distance from us, and which consequently do not exist in the mind; it being absurd that those things which are seen at the distance of several miles, should be as near to us as our own thoughts. In answer to this, I desire it may be considered that in a dream we do oft perceive things as existing at a great distance off, and yet, for all that, those things are acknowledged to have their existence only in the mind.

"But for the fuller clearing of this point, it may be worth while to consider how it is that we perceive distance and things placed at a distance by sight. For that we should in truth see external space and bodies actually existing in it, some nearer, others further off, seems to carry with it some opposition to what hath been said of their existing nowhere without the mind. The consideration of this difficulty it was that gave birth to my 'Essay towards the New Theory of Vision,' which was published not long since, wherein it is shown that distance or outness is neither immediately of itself perceived by sight, nor yet apprehended or judged of by lines and angles or anything that hath any necessary connection with it; but that it is only suggested to our thoughts by certain visible ideas and sensations attending vision, which, in their own nature, have no manner of similitude or relation either with distance, or with things placed at a distance; but by a connection taught us by experience, they come to signify and suggest them to us, after the same manner, that words of any language suggest the ideas they are made to stand for; insomuch that a man born blind and afterwards made to see, would not, at first sight, think the things he saw to be without his mind or at any distance from him."

The key-note of the Essay to which Berkeley refers in this passage is to be found in an italicized paragraph of section 127.

"*The extensions, figures, and motions perceived by sight are specifically distinct from the ideas of touch called by the same names; nor is there any such thing as an idea or kind of idea common to both senses.*"

It will be observed that this proposition expressly declares that extension, figure, and motion, and consequently distance, are immediately perceived by sight as well as by touch; but that visual distance, extension, figure, and motion, are totally different in quality from the ideas of the same name obtained through the sense of touch. And other passages leave no doubt that such was Berkeley's meaning. Thus in the 112th section of the same Essay, he carefully defines the two kinds of distance, one visual, the other tangible.

"By the distance between any two points nothing more is meant than the number of intermediate points. If the given points are visible, the distance between them is marked out by the number of interjacent visible points; if they are tangible, the distance between them is a line consisting of tangible points."

Again, there are two sorts of magnitude or extension :—

"It has been shown that there are two sorts of objects apprehended by sight, each whereof has its distinct magnitude or extension: the one properly tangible, *i. e.* to be perceived and measured by touch, and not immediately falling under the sense of seeing; the other properly and immediately visible, by mediation of which the former is brought into view."—§ 55.

But how are we to reconcile these passages with others which will be perfectly familiar to every reader of the 'New Theory of Vision'? As, for example :—

"It is, I think, agreed by all, that distance of itself, and immediately, cannot be seen."—§ 2.

"Space or distance, we have shown, is not otherwise the object of sight than of hearing."—§ 130.

"Distance is in its own nature imperceptible, and yet it is perceived by sight. It remains, therefore, that it is brought into view by means of some other idea, that is itself immediately perceived in the act of vision."—§ 11.

"Distance or external space."—§ 155.

The explanation is quite simple, and lies in the fact that Berkeley uses the word "distance" in three senses. Sometimes he employs it to denote visible distance, and then he restricts it to distance in two dimensions, or simple extension. Sometimes he means tangible distance in two dimensions; but most commonly he intends to signify tangible distance in the third dimension. And it is in this sense that he employs "distance" as the equivalent of "space." Distance in two dimensions is, for Berkeley, not space, but extension. By taking a pencil and interpolating the words "visible" and "tangible" before "distance" wherever the context renders them necessary, Berkeley's statements may be made perfectly consistent; though he has not always extricated himself from the entanglement caused by his own loose phraseology, which rises to a climax in the last ten sections of the 'Theory of Vision,' in which he endeavours to prove that a pure intelligence able to see, but devoid of the sense of touch, could have no idea of a plane figure. Thus he says in section 156 :—

"All that is properly perceived by the visual faculty amounts to no more than colours with their variations and different proportions of light and shade; but the perpetual mutability and fleetingness of those immediate objects of sight render them incapable of being managed after the manner of geometrical figures, nor is it in any degree useful that they should. It is true there be divers of them perceived at once, and more of some and less of others; but accurately to compute their magnitude, and assign precise determinate proportions between things so variable and inconstant, if we suppose it possible to be done, must yet be a very trifling and insignificant labour."

If, by this, Berkeley means that by vision alone, a straight line cannot be distinguished from a curved one, a circle from a square, a long line from a short one, a large angle from a small one, his position is surely absurd in itself and contradictory to his own previously cited admissions; if he only means, on the other hand, that his pure spirit could not get very far on in his geometry, it may be true or not; but it is in contradiction with his previous assertion, that such a

pure spirit could never attain to know as much as the first elements of plane geometry.

Another source of confusion, which arises out of Berkeley's insufficient exactness in the use of language, is to be found in what he says about solidity in discussing Molyneux's problem, whether a man born blind, and having learned to distinguish between a cube and a sphere, could, on receiving his sight, tell the one from the other by vision. Berkeley agrees with Locke that he could not, and adds the following reflection :—

"Cube, sphere, table are words he has known applied to things perceivable by touch, but to things perfectly intangible he never knew them applied. Those words in their wonted application always marked out to his mind bodies or solid things which were perceived by the resistance they gave. But there is no solidity, no resistance or protrusion perceived by sight."

Here "solidity" means resistance to pressure, which is apprehended by the muscular sense; but when in section 154 Berkeley says of his pure intelligence—

"It is certain that the aforesaid intelligence could have no idea of a solid or quantity of three dimensions, which follows from its not having any idea of distance"—

he refers to that notion of solidity which may be obtained by the tactile sense, without the addition of any notion of resistance in the solid object; as, for example, when the finger passes lightly over the surface of a billiard ball.

Yet another source of difficulty in clearly understanding Berkeley arises out of his use of the word "outness." In speaking of touch he seems to employ it indifferently, both for the localization of a tactile sensation in the sensory surface, which we really obtain through touch; and for the notion of corporeal separation, which is attained by the association of muscular and tactile sensations. In speaking of sight, on the other hand, Berkeley employs "outness" to denote corporeal separation.

When due allowance is made for the occasional looseness and ambiguity of Berkeley's terminology, and the accessories are weeded out of the essential parts of his famous Essay, his views may, I believe, be fairly and accurately summed up in the following propositions:—

1. The sense of touch gives rise to ideas of extension, figure, magnitude, and motion.

2. The sense of touch gives rise to the idea of "outness," in the sense of localization.

3. The sense of touch gives rise to the idea of resistance, and thence to that of solidity, in the sense of impenetrability.

4. The sense of touch gives rise to the idea of "outness," in the sense of distance in the third dimension, and thence to that of space, or geometrical solidity.

5. The sense of sight gives rise to ideas of extension of figure, magnitude, and motion.

6. The sense of sight does not give rise to the idea of "outness,"

in the sense of distance in the third dimension, nor to that of geometrical solidity, no visual idea appearing to be without the mind, or at any distance off (§§ 43, 50).

7. The sense of sight does not give rise to the idea of mechanical solidity.

8. There is no likeness whatever between the tactile ideas called extension, figure, magnitude, and motion, and the visual ideas which go by the same names; nor are any ideas common to the two senses.

9. When we think we see objects at a distance, what really happens is that the visual picture suggests that the object seen has tangible distance; we confound the strong belief in the tangible distance of the object with actual sight of its distance.

10. Visual ideas, therefore, constitute a kind of language, by which we are informed of the tactile ideas which will or may arise in us.

Taking these propositions into consideration *seriatim*, it may be assumed that everyone will assent to the first and second; and that for the third and fourth we have only to include the muscular sense under the name of sense of touch, as Berkeley did, in order to make it quite accurate. Nor is it intelligible to me that anyone should explicitly deny the truth of the fifth proposition, though some of Berkeley's supporters, less careful than himself, have done so. Indeed, it must be confessed that it is only grudgingly, and as it were against his will, that Berkeley admits that we obtain ideas of extension, figure, and magnitude by pure vision, and that he more than half retracts the admission; while he absolutely denies that sight gives us any notion of outness in either sense of the word, and even declares that "no proper visual idea appears to be without the mind, or at any distance off." By "proper visual ideas," Berkeley denotes colours, and light, and shade; and, therefore, he affirms that colours do not appear to be at any distance from us. I confess that this assertion appears to me to be utterly unaccountable. I have made endless experiments on this point, and by no effort of the imagination can I persuade myself, when looking at a colour, that the colour is in my mind, and not at a "distance off," though of course I know perfectly well, as a matter of reason, that colour is subjective. It is like looking at the sun setting, and trying to persuade oneself that the earth appears to move and not the sun, a feat I have never been able to accomplish. Even when the eyes are shut, the darkness of which one is conscious, carries with it the notion of outness. One looks, so to speak, into a dark space. Common language expresses the common experience of mankind in this matter. A man will say that a smell is in his nose, a taste in his mouth, a singing in his ears, a creeping or a warmth in his skin; but if he is jaundiced, he does not say that he has yellow in his eyes, but that everything looks yellow; and if he is troubled with *müscæ volitantes*, he says, not that he has specks in his eyes, but that he sees specks dancing before his eyes. In fact, it appears to me that it is the special peculiarity of visual sensations, that they invariably give rise

to the idea of remoteness, and that Berkeley's dictum ought to be reversed. For I think that anyone who interrogates his consciousness carefully will find that "every proper visual idea" appears to be without the mind and at a distance off.

Not only does every *visibile* appear to be remote, but it has a position in external space, just as a *tangibile* appears to be superficial and to have a determinate position on the surface of the body. Every *visibile*, in fact, appears (approximately) to be situated upon a line drawn from it to the point of the retina on which its image falls. It is referred outwards, in the general direction of the pencil of light by which it is rendered visible, just as, in the experiment with the stick, the *tangibile* is referred outwards to the end of the stick.

It is for this reason that an object, viewed with both eyes, is seen single and not double. Two distinct images are formed, but each image is referred to that point at which the two optic axes intersect; consequently, the two images exactly cover one another, and appear as completely one as any other two exactly similar superimposed images would be. And it is for the same reason, that, if the ball of the eye is pressed upon at any point, a spot of light appears apparently outside the eye, and in a region exactly opposite to that in which the pressure is made.

But while it seems to me that there is no reason to doubt that the extradition of sensation is more complete in the case of the eye than in that of the skin, and that corporeal distinctness, and hence space, are directly suggested by vision, it is another, and a much more difficult question, whether the notion of geometrical solidity is attainable by pure vision; that is to say, by a single eye, all the parts of which are immovable. However this may be for an absolutely fixed eye, I conceive there can be no doubt in the case of an eye that is movable and capable of adjustment. For, with the movable eye, the muscular sense comes into play in exactly the same way as in the movable hand, and the notion of change of place, *plus* the sense of effort, gives rise to a conception of visual space, which runs exactly parallel with that of tangible space. When two movable eyes are present, the notion of space of three dimensions is obtained in the same way as it is by the two hands, but with much greater precision.

And if, to take a case similar to one already assumed, we suppose a man deprived of every sense except vision, and of all motion except that of his eyes, it surely cannot be doubted that he would have a perfect conception of space; and indeed a much more perfect conception than he who possessed touch alone without vision. But of course our touchless man would be devoid of any notion of resistance; and hence space, for him, would be altogether geometrical and devoid of body.

And here another curious consideration arises, what likeness, if any, would there be between the visual space of the one man, and the tangible space of the other?

Berkeley, as we have seen (in the eighth propesition), declares

that there is no likeness between the ideas given by sight and those given by touch; and one cannot but agree with him, so long as the term ideas is restricted to mere sensations. Obviously, there is no more likeness between the feel of a surface and the colour of it, than there is between its colour and its smell. All simple sensations, derived from different senses, are incommensurable with one another, and only gradations of their own intensity are comparable. And thus so far as the primary facts of sensation go, visual figure and tactile figure, visual magnitude and tactile magnitude, visual motion and tactile motion, are truly unlike, and have no common term. But when Berkeley goes further than this, and declares that there are no "ideas" common to the "ideas" of touch and those of sight, it appears to me that he has fallen into a great error, and one which is the chief source of his paradoxes about geometry.

Berkeley in fact employs the word "idea" in this instance to denote two totally different classes of feelings, or states of consciousness. For these may be divided into two groups, the primary feelings which exist in themselves and without relation to any other, such as pleasure and pain, desire, and the simple sensations obtained through the sensory organs; and the secondary feelings, which express those relations of primary feelings which are perceived by the mind; and the existence of which, therefore, implies the pre-existence of at least two of the primary feelings. Such are likeness and unlikeness in quality, quantity, or form; succession and contemporaneity; contiguity and distance; cause and effect; motion and rest.

Now it is quite true that there is no likeness between the primary feelings which are grouped under sight and touch; but it appears to me wholly untrue, and indeed absurd, to affirm that there is no likeness between the secondary feelings which express the relations of the primary ones.

The relation of succession perceived between the visible taps of a hammer, is, to my mind, exactly like the relation of succession between the tangible taps; the unlikeness between red and blue is a mental phenomenon of the same order as the unlikeness between rough and smooth. Two points visibly distant are so, because one or more units of visible length (*minima visibilia*) are interposed between them; and as two points tangibly distant are so, because one or more units of tangible length (*minima tangibilia*) are interposed between them, it is clear that the notion of interposition of units of sensibility, or *minima sensibilia*, is an idea common to the two. And whether I see a point move across the field of vision towards another point, or feel the like motion, the idea of the gradual diminution of the number of sensible units between the two points appears to me to be common to both kinds of motion.

Hence I conceive that though it be true that there is no likeness between the primary feelings given by sight and those given by touch, yet there is a complete likeness between the secondary feelings aroused by each sense.

Indeed, if it were not so, how could logic, which deals with those forms of thought which are applicable to every kind of subject-matter, be possible? How could numerical proportion be as true of *visibilia*, as of *tangibilia*, unless there were some ideas common to the two? And to come directly to the heart of the matter, is there any more difference between the relations between tangible sensations which we call place and direction, and those between visible sensations which go by the same name, than there is between those relations of tangible and visible sensations which we call succession? And if there be none, why is geometry not just as much a matter of *visibilia* as of *tangibilia*?

Moreover, as a matter of fact, it is certain that the muscular sense is so closely connected with both the visual and the tactile senses, that, by the ordinary laws of association, the ideas which it suggests must needs be common to both.

From what has been said it will follow that the ninth proposition falls to the ground; and that vision, combined with the muscular sensations produced by the movement of the eyes, gives us as complete a notion of corporeal separation and of distance in the third dimension of space, as touch, combined with the muscular sensations produced by the movements of the hand, does. The tenth proposition seems to contain a perfectly true statement, but it is only half the truth. It is no doubt true that our visual ideas are a kind of language by which we are informed of the tactile ideas which may or will arise in us; but this is true, more or less, of every sense in regard to every other. If I put my hand in my pocket, the tactile ideas which I receive prophesy quite accurately what I shall see—whether a bunch of keys or half-a-crown—when I pull it out again; and the tactile ideas are, in this case, the language which informs me of the visual ideas which will arise. So with the other senses; olfactory ideas tell me I shall find the tactile and visual phenomena called violets, if I look for them; taste tells me that what I am tasting will, if I look at it, have the form of a clove; and hearing warns me of what I shall, or may, see and touch every minute of my life.

But while the 'New Theory of Vision' cannot be considered to possess much value in relation to the immediate object its author had in view, it had a vastly important influence in directing attention to the real complexity of many of those phenomena of sensation, which appear at first to be simple. And even if Berkeley was, as I imagine he was, quite wrong in supposing that we do not see space, the contrary doctrine makes quite as strongly for his general view, that space can be conceived only as something thought by a mind.

The last of Locke's "primary qualities" which remain to be considered is mechanical solidity or impenetrability. But our conception of this is derived from the sense of resistance to our own effort, or active force, which we meet with, in association with sundry tactile or visual phenomena; and, undoubtedly, active force is inconceivable except as a state of consciousness. This may sound paradoxical; but let anyone try to realize what he means by the mutual attraction of

two particles, and I think he will find, either, that he conceives them simply as moving towards one another at a certain rate, in which case he only pictures motion to himself, and leaves force aside; or, that he conceives each particle to be animated by something like his own volition, and to be pulling as he would pull. . . . .

To sum up. If the materialist affirms that the universe and all its phenomena are resolvable into matter and motion, Berkeley replies, True; but what you call matter and motion are known to us only as forms of consciousness; their being is to be conceived or known; and the existence of a state of consciousness, apart from a thinking mind, is a contradiction in terms.

I conceive that this reasoning is irrefragable. And therefore, if I were obliged to choose between absolute materialism and absolute idealism, I should feel compelled to accept the latter alternative. And indeed, upon this point Locke does, practically, go as far in the direction of idealism as Berkeley, when he admits that "the simple ideas we receive from sensation and reflection are the boundaries of our thoughts, beyond which the mind, whatever efforts it would make, is not able to advance one jot."—Book II., chap. xxiii., § 29.

But Locke adds, "Nor can it make any discoveries when it would pry into the nature and hidden causes of these ideas."

Now, from this proposition, the thorough materialists dissent as much, on the one hand, as Berkeley does, upon the other hand.

The thorough materialist asserts that there is a something which he calls the "substance" of matter; that this something is the cause of all phenomena, whether material or mental; that it is self-existent, and eternal, and so forth.

Berkeley, on the contrary, asserts with equal confidence that there is no substance of matter, but only a substance of mind, which he terms spirit; that there are two kinds of spiritual substance, the one eternal and uncreated, the substance of the Deity, the other created, and, once created, naturally eternal; that the universe as known to created spirits, has no being in itself, but is the result of the action of the substance of the Deity on the substance of those spirits.

In contradiction to which bold assertion Locke affirms that we simply know nothing about substance of any kind.*

"So that if anyone will examine himself concerning his notion of pure substance in general, he will find he has no other idea of it at all, but only a supposition of he knows not what support of such qualities, which are capable of producing simple ideas in us, which qualities are commonly called accidents.

"If anyone should be asked, what is the subject wherein colour or weight inheres? he would have nothing to say but the solid extended parts; and if he were demanded what is it that solidity and extension inhere in? he would not be in much better case than the Indian before mentioned, who urging that the world was supported by a great elephant, was asked what the elephant rested on? to

* Berkeley virtually makes the same confession of ignorance, when he admits that we can have no idea or notion of a spirit ('Principles of Human Knowledge,' § 138); and the way in which he tries to escape the consequences of this admission, is a splendid example of the floundering of a mired logician.

which his answer was, a great tortoise. But being again pressed to know what gave support to the broad-backed tortoise? replied, something, he knew not what. And thus here, as in all other cases when we use words without having clear and distinct ideas, we talk like children, who, being questioned what such a thing is, readily give this satisfactory answer, that it is something; which in truth signifies no more when so used either by children or men, but that they know not what, and that the thing they pretend to talk and know of is what they have no distinct idea of at all, and are, so, perfectly ignorant of it and in the dark. The idea then, we have, to which we give the general name substance, being nothing but the supposed but unknown support of those qualities we find existing, which we imagine cannot exist *sine re substante*, without something to support them, we call that support *substantia*, which, according to the true import of the word, is, in plain English, standing under or upholding." *

I cannot but believe that the judgment of Locke is that which philosophy will accept as her final decision.

Suppose that a piano were conscious of sound and of nothing else? It would become acquainted with a system of nature entirely composed of sounds, and the laws of nature would be the laws of melody and of harmony. It might acquire endless ideas of likeness and unlikeness, of succession, of similarity and dissimilarity, but it could attain to no conception of space, of distance, or of resistance, or of figure, or of motion.

The piano might then reason thus: All my knowledge consists of sounds and the perception of the relations of sounds; now the being of sound is to be heard; and it is inconceivable that the existence of the sounds I know, should depend upon any other existence than that of the mind of a hearing being.

This would be quite as good reasoning as Berkeley's, and very sound and useful, so far as it defines the limits of the piano's faculties. But for all that, pianos have an existence quite apart from sounds, and the auditory consciousness of our speculative piano would be dependent, in the first place, on the existence of a "substance" of brass, wood, and iron, and, in the second, on that of a musician. But of neither of these conditions of the existence of his consciousness would the phenomena of that consciousness afford him the slightest hint.

So that while it is the summit of human wisdom to learn the limit of our faculties, it may be wise to recollect that we have no more right to make denials, than to put forth affirmatives about what lies beyond that limit. Whether either mind, or matter, have a "substance" or not, is a problem which we are incompetent to discuss; and it is just as likely that the common notions about the matter should be correct as any others. Indeed, Berkeley himself makes Philonous wind up his discussions with Hylas in a couple of sentences which aptly express this conclusion:—

"You see, Hylas, the water of yonder fountain, how it is forced upwards in a round column to a certain height, at which it breaks and falls back into the basin from whence it rose: its ascent as well as its descent proceeding from the same uniform law or principle of gravitation. Just so, the same principles which, at first view, lead to scepticism, pursued to a certain point, bring men back to common sense."

[T. H. H.]

* Locke, 'Human Understanding,' Book II., chap. xxiii., § 2.

WEEKLY EVENING MEETING,

Friday, May 26, 1871.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

W. J. MACQUORN RANKINE, C.E. LL.D. F.R.S.

On Sea Waves.

THE speaker in the first place gave a summary, illustrated by diagrams and machines, of existing knowledge of the mode of motion of water in waves, and of the geometrical and dynamical laws which govern the relations between the depth of disturbance of the water, the velocity of advance of waves, their periodic time, and their length. He referred to the experimental and theoretical researches of previous authors on the subject, such as the Webers, Airy, Scott Russell, Caligny, &c.

He then explained the principle, of which Mr. Froude was the first to point out the importance, that the action of water agitated by waves upon a ship tends to make her perform the motions which would have been performed in her absence by the mass of water that she displaces. In still water, the forces of gravity and of buoyancy tend to keep the ship upright, and if she has been heeled over, to restore her to the upright position, and that tendency constitutes the *statical stability* or *stiffness* of the ship. Amongst waves the same forces, combined with the reactions due to the heaving motions of the water and of the ship, tend to place her in the position called *upright to the wave surface;* that is, with her originally vertical axis normal to the wave surface. If the ship yielded passively to that tendency, like a broad and shallow raft, she would accompany the waves in their rolling; and thus, a ship having great *stiffness* may be very deficient in *steadiness*. Every ship has, like a pendulum, a natural period of rolling, depending on her stiffness, or tendency to right herself, and her moment of inertia, being a quantity depending on the distribution of her mass. Stiffness tends to shorten, and inertia to lengthen, the period. It was shown in 1862, by Mr. Froude, that the greatest unsteadiness and the greatest danger of being overturned take place when the periodic times of rolling of the ship and of the waves are equal; for then each successive wave adds to the extent of roll; and if the coincidence of the periods were exact, the ship would inevitably be overturned in the end.

In the course of the present spring it has been pointed out that in well-designed ships a safeguard exists against the occurrence of such disasters. It is well known that no pendulum is absolutely isochronous; but great oscillations occupy a longer time than small oscillations. In like manner, no ship is absolutely isochronous in her natural rolling;

but great angles of roll occupy longer periods than small.* Hence, supposing a ship to encounter waves of a period equal or nearly equal to her own natural period for small angles of roll, her angle of rolling is at first progressively increased; but at the same time her natural periodic time of rolling is increased, until it is no longer equal or nearly equal to the periodic time of the waves; and thus she in a manner *eludes* the danger arising from coincidence of periods. In order, however, that this safeguard may act efficiently, it is essential that the natural period of the ship for the smallest angles of roll should not be less than the period of the waves; otherwise the first effect of the progressive increase of angle will be, not to destroy, but to produce coincidence of period; and the result will be great unsteadiness of motion, and possibly great danger.

The speaker described the above principles as being the latest additions to our knowledge of the theory of the relations between ships and sea-waves; and he illustrated them by means of experiments on a machine so constructed as to imitate the dynamical condition of a ship rolling amongst waves.

[W. J. M. R.]

WEEKLY EVENING MEETING,

Friday, June 2, 1871.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Thomas Andrews, M.D. F.R.S.

VICE-PRESIDENT OF QUEEN'S COLLEGE, BELFAST,

On the Gaseous and Liquid States of Matter.

The liquid state of matter forms a link between the solid and gaseous states. This link is, however, often suppressed, and the solid passes directly into the gaseous or vaporous form. In the intense cold of an Arctic winter hard ice will gradually change into transparent vapour without previously assuming the form of water. Carbonic acid snow passes rapidly into gas when exposed to the air, and can with diffi-

* *Note* (added 2nd June).—An exception to this rule exists in the case of that form of ship known as the "Symondite," in which the sides flare out at and near the water-line, so as to make the stiffness increase faster than the angle of heel. In such ships the period of rolling *shortens* when the angle increases; and thus the well-known unsteadiness of large vessels of that model is accounted for. In a small boat, whose natural periodic time for the smallest angle of roll is shorter than that of any of the waves which she encounters, the Symondite model does not promote unsteadiness; for the shortening of the natural period of rolling removes it farther from coincidence with the period of the waves.

culty be liquefied in open tubes. Its boiling point, as Faraday has shown, presents the apparent anomaly of being lower in the thermometric scale than its melting point; a statement less paradoxical than it may at first appear, if we remember that water can exist as vapour at temperatures far lower than those at which it can exist as liquid. Whether the transition be directly from solid to gaseous, or from solid to liquid, and from liquid to gaseous, a marked change of physical properties occurs at each step or break, and heat is absorbed, as was proved long ago by Black, without producing elevation of temperature. Many solids and liquids will for this reason maintain a low temperature, even when surrounded by a white hot atmosphere, and the remarkable experiment of solidifying water, and even mercury, on a red-hot plate, finds thus an easy explanation. The term spheroidal state, when applied to water floating on a cushion of vapour over a red-hot plate, is however apt to mislead. The water is not here in any peculiar state. It is simply water evaporating rapidly at a few degrees below its boiling point, and all its properties, even those of capillarity, are the properties of ordinary water at 96·5 C. The interesting phenomena exhibited under these conditions are due to other causes, and not to any new or peculiar state of the liquid itself. The fine researches of Dalton upon vapours, and the memorable discovery by Faraday of the liquefaction of gases by pressure alone, finished the work which Black had begun. Our knowledge of the conditions under which matter passes abruptly from the gaseous to the liquid, and from the liquid to the solid state, may now be regarded as almost complete.

In 1822 Cagniard de La Tour made some remarkable experiments, which still bear his name, and may be regarded as the starting-point of the investigations which form the chief subject of this address. Cagniard de La Tour's first experiments were made in a small Papin's digester, constructed from the thick end of a gun-barrel, into which he introduced a little alcohol and also a small quartz ball, and firmly closed the whole. On heating the gun-barrel with its contents over an open fire, and observing from time to time the sound produced by the ball when the apparatus was shaken, he inferred that after a certain temperature was attained the liquid had disappeared. He afterwards succeeded in repeating the experiment in glass tubes, and obtained the following results:—An hermetically-sealed glass tube, containing sufficient alcohol to occupy two-fifths of its capacity, was gradually heated, when the liquid was seen to dilate, and its mobility at the same time to become gradually greater. After attaining to nearly twice its original volume, the liquid completely disappeared, and was converted into a vapour so transparent that the tube appeared to be quite empty. On allowing the tube to cool, a very thick cloud was formed, after which the liquid reappeared in its former state.

It is singular that in this otherwise accurate description, Cagniard de La Tour should have overlooked the most remarkable appearance of all, the moving or flickering striæ, which fill the tube when, after

heating it considerably, the temperature is quickly lowered. This phenomenon was first described by myself in 1863, as it is seen in carbonic acid, which has been partially liquefied by pressure, and afterwards heated a little above 31°. It may be observed on a larger scale and to great advantage by heating such liquids as sulphurous acid or ether in hermetically-sealed tubes.

The experiments whose results I am about to describe have occupied me for a period of fully ten years; they involved the construction of novel forms of apparatus, in which the properties of matter might be studied under varied conditions of temperature and pressure, such as had never been realized before. In my earlier attempts I endeavoured, as others had already done, to use the expansive force of the mixed gases which are disengaged in the electrolysis of water; and I was able in this way to obtain pressures of 150 atmospheres and even more in glass tubes; but the method was in many respects defective, and more than one dangerous explosion occurred, so that I eventually abandoned it.

In the apparatus finally adopted, the gas to be compressed is enclosed in a long glass tube, of which the greater part of the length, or about 450 millimetres, has a capillary bore, and the remainder, about 150 millimetres, an internal diameter of 2 millimetres. The free capillary end is sealed, while the gas in a pure and dry state is passing through; while at the other end the gas is confined by a movable column of mercury. The details of the method by which this is accomplished will be found in the Bakerian lecture for 1869, to which I must also refer for an account of the process by which the original volume of the gas at the freezing point of water and under one atmosphere of pressure was determined, and also the volumes of the same gas deduced from the observed measurements when it was compressed at different pressures in the capillary tube.

A conical protuberance on the capillary part of the tube, a little above its junction with the wider part, corresponded as nearly as possible with a hollow cone in a stout brass flange, the joint being rendered perfectly tight by careful packing. The body of the apparatus consisted of two cold-drawn copper tubes of great strength, to the ends of which four massive brass flanges were firmly attached. Two corresponding flanges or end pieces, each carrying a fine steel screw packed with great care, were bolted on the lower flanges. The success of the experiments depended greatly on the packing of this screw. It was effected by means of a number of leather washers, tightly pressed down and saturated *in vacuo* with melted lard. The apparatus was now filled with water; the flanges with the glass tubes, one containing the gas to be examined, the other air or hydrogen to act as a manometer or measure of the pressure, were bolted down upon the upper flanges of the copper tubes. The joints had always leather washers interposed; and when sufficiently tightened, they resisted any pressure which could be applied, even for an indefinite time. The two copper tubes were

connected by a fine horizontal tube, so that the whole of the interior of the apparatus was in free communication. The pressure was obtained by screwing one or other of the steel screws into the water. I have recently had the apparatus constructed of iron and filled with mercury. As mercury is much less compressible than water, the same length of screw produces a greater pressure on the interior of the apparatus, even with a larger cavity. There are other advantages in this form of the apparatus which I hope will facilitate future research. The objection to it is its extreme sensitiveness to changes of temperature, so that a variation of $\frac{1}{100}$th of a degree alters the internal pressure by several atmospheres.

In the actual experiments the gas under examination does not come into view till it has entered the capillary tube, and is exposed to a pressure of thirty or forty atmospheres. The limit of the pressure which can be obtained has hitherto been the capacity of resistance of the glass tubes to bursting. Fine thermometer glass tubes of white glass will frequently burst when exposed to a pressure of little more than 100 atmospheres; but green glass tubes of good quality are much stronger, and will easily bear a pressure of 300 atmospheres. One of the strongest forms of glass capillary tube for resisting internal pressure is obtained by drawing out a thick green glass tube, heated to softening, till it becomes so fine as to be flexible. Tubes of this kind can easily be drawn out at the blowpipe table, and obtained of very uniform bore. I have compressed air in such tubes to $\frac{1}{400}$th of its ordinary volume without bursting the tubes.

Two rectangular brass cases, closed before and behind with plate-glass, surround, one the manometer, and the other the tube containing the gas to be examined, and allow them to be maintained at any required temperature by the flow of a stream of water. The manometer was maintained as nearly as possible at the temperature of the apartment; the tube containing the gas, on the contrary, was maintained at different temperatures, according to the object in view. The following observations, published in 1863, contain the results of my earliest experiments on this subject:—"On partially liquefying carbonic acid by pressure alone, and gradually raising the temperature at the same time to 88° Fahr., the surface of demarcation between the liquid and gas becomes fainter, loses its curvature, and at last disappears. The space is then occupied by a homogeneous fluid, which exhibits when the pressure is suddenly diminished or the temperature slightly lowered, a peculiar appearance of moving or flickering striæ throughout its entire mass. At temperatures above 88° no apparent liquefaction, or separation into two distinct forms of matter, could be effected, even when a pressure of 300 or 400 atmospheres was applied. Nitrous oxide gave analogous results."

The flickering striæ referred to can be admirably shown, as I mentioned before, in hermetically-sealed tubes of strong glass, partially filled with such liquids as sulphurous acid or ether. The liquid must in the first instance be heated a few degrees above what I have

designated the "critical" point. The appearances exhibited by the ascending and descending sheets of matter of unequal density are most remarkable, but must be seen in order to be understood. They only occur in this striking form in fluids heated a little above the critical point, and are produced by the great changes of density which slight variations of pressure or temperature produce in this case. They are always a clear proof that the matter in the tube is homogeneous, and that we have not liquid and gas in presence of one another. These striæ are in short only an extraordinary development of the movements seen in ordinary liquids and gases when they are heated from below. The experiments to be immediately described will explain their great intensity above the critical point.

When the temperature falls below the critical point, the formation of a cloud indicates that we have now heterogeneous matter in the tube, fine drops of liquid in presence of a gas. We must take care, however, not to suppose that a cloud *necessarily* precedes the formation of true liquid. If the pressure be sufficiently great no cloud of any kind will form.

I now proceed to describe the general results of the experiments upon carbonic acid. If a certain volume of carbonic acid at the temperature of 13°·1 and under a pressure of one atmosphere be exposed to a gradually-increasing pressure, its volume will steadily diminish, but at a faster rate than according to Boyle's law, till at the pressure of 48·9 atmospheres its volume is reduced to about $\frac{1}{31}$ of the original volume at one atmosphere. Liquefaction now begins and continues with very slight augmentation of pressure, the necessity for which I traced to the presence of a minute quantity of air (about $\frac{1}{500}$th part) in the carbonic acid. On augmenting the pressure after liquefaction, the volume slowly diminished, but at a much faster rate than in the case of ordinary liquids. Later experiments carried to much higher pressures have fully confirmed this result. At 21°·5 similar results were obtained, but a pressure of nearly 60 atmospheres was required before liquefaction began.

At 30°·9 C., or 87°·7 Fahr., the critical point of temperature is reached. It is the temperature at which liquid ceases to be formed under any pressure. At a temperature a little below this point the surface of separation between liquid and gas becomes very faint and loses its curvature, the density and other physical properties of the liquid and gas being now identical and the tube filled with homogeneous matter. If the temperature and pressure be kept steady, no evidence of heterogeneity will be obtained by optical tests under the most varied conditions of volume.

If we now follow the course of a given volume of carbonic acid gas at 31°·1, or 0°·2 above the critical point, we shall find that its course resembles that of the gas at lower temperatures till the volume is reached at which liquefaction might be expected to begin. A rapid but not (as in the case of the formation of liquid) abrupt fall then supervenes, after which the carbonic acid undergoes a slow diminution

of volume as the pressure augments. The curves, which are here exhibited as they were represented in the Bakerian lecture, illustrate very clearly these statements. We have thus carbonic acid at 0°·2 above the critical point, and at a pressure of 73 atmospheres behaving very nearly as if it liquefied. At this pressure an augmentation of only $\frac{1}{37}$th of the entire pressure diminishes the volume of the carbonic acid to about one half. Yet during the whole of this fall, no evidence of heterogeneity, or of two states of matter present together in the tube, could at any period be obtained. Carbonic acid at this temperature of 31°·1, and under a pressure of 75 atmospheres, behaves much more as a liquid than as a gas when the pressure is either augmented or diminished; yet it never exhibits under any conditions the characteristic properties of the liquid state; that is to say, no surface of separation is formed by change of pressure, nor will it collect into drops and form a cloud.

At 32°·5 the fall, when liquefaction might be expected, is less abrupt than at 31°·1; and at 35°·5, although still manifest, it is further reduced. At 48°·1 the fall shown at lower temperatures can no longer be distinctly observed, and the curve representing the change of volume approximates to that of a perfect gas. There can be little, if any, doubt that at a higher temperature carbonic acid would behave under augmenting pressures nearly as nitrogen or hydrogen.*

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* These different modes of passing from the gaseous to the liquid state are admirably illustrated by a solid model constructed by Prof. J. Thomson, which was exhibited at the lecture. I have been favoured by Prof. Thomson with the following description of this model:—

"The model combines Dr. Andrews' experimental results in a manner tending to show clearly their mutual correlation. It consists of a curved surface referred to three axes of rectangular co-ordinates, and formed so that the three co-ordinates of each point in the curved surface represent, for any given mass of carbonic acid, a pressure, a temperature, and a volume, which can co-exist in that mass.

"In Dr. Andrews' diagram of curves, published in his paper in the 'Transactions of the Royal Society for 1869,' p. 583, the experimental results, for each of several temperatures experimented on, are combined in the form of a plane curved line referred to two axes of rectangular co-ordinates. The curved surface in the model is obtained by placing these curved lines with their planes parallel to one another, and separated by intervals proportional to the differences of the temperatures to which the curves severally belong, and with the origins of co-ordinates of the curves situated in a straight line perpendicular to their planes, and with the axes of co-ordinates of all of them parallel in pairs to one another, and by cutting the curved surface out so as to pass through those curved lines smoothly or evenly.

"The curved surface so obtained exhibits in a very obvious way the remarkable phenomena of the voluminal conditions at and near the critical point of temperature and pressure in comparison with the voluminal conditions throughout other parts of the indefinite range of gradually varying temperatures and pressures. This curved surface also helps to afford a clear view of the nature and meaning of the continuity of the liquid and gaseous states of matter. It does so by its own obvious continuity throughout the expanse to which it might be extended round the outside of the critical point in receding from the range of the points of pressure and temperature where an abrupt change of volume can occur by gasification or condensation. On the curved surface in the model, Dr. Andrews' curves for the

I have frequently exposed carbonic acid, without making precise measurements, to much higher pressures than any of the foregoing, and have made it pass without break or interruption from what is regarded by every one as the gaseous state, to what is, in like manner, universally regarded as the liquid state. Take, for example, a given volume of carbonic acid gas at 50° C., or at a higher temperature, and expose it to increasing pressure till 150 atmospheres have been reached. In this process its volume will steadily diminish as the pressure augments, and no sudden diminution of volume, without the application of external pressure will occur at any stage of it. When the full pressure has been applied, let the temperature be allowed to fall till the carbonic acid has reached the ordinary temperature of the atmosphere. During the whole of this operation no breach of continuity has occurred. It begins with a gas, and by a series of gradual changes, presenting nowhere any abrupt alteration of volume or sudden evolution of heat, it ends with a liquid. The closest observation fails to discover anywhere indications of a change of condition in the carbonic acid, or evidence, at any period of the process, of part of it being in one physical state and part in another. That the gas has actually changed into a liquid would, indeed, never have been suspected, had it not shown itself to be so changed by entering into ebullition on the removal of the pressure. For convenience, this process has been divided into two stages, the compression of the carbonic acid and its subsequent cooling; but these operations might have been performed simultaneously, if care were taken so to arrange the application of the pressure and the rate of cooling, that the pressure should not be less than 76 atmospheres when the carbonic acid had cooled to 31°.

We are now prepared for the consideration of the following important question. What is the condition of carbonic acid when it passes, at temperatures above 31°, from the gaseous state down to the volume of the liquid, without giving evidence at any part of the process of liquefaction having occurred? Does it continue in the gaseous state, or does it liquefy, or have we to deal with a new condition of matter? If the experiment were made at 100°, or at a higher temperature, when all indications of a fall had disappeared, the probable answer which would be given to this question is that the gas preserves its gaseous condition during the compression; and few would hesitate to declare this statement to be true, if the pressure were applied to such gases as hydrogen or nitrogen. On the other hand, when the experiment is made with carbonic acid at temperatures a little above 31°, the great fall which occurs at one period of

temperatures 13°·1, 21°·5, 31°·1, 35°·5, and 48°·1 centigrade, from which it was constructed, are shown drawn in their proper places. The model admits of easily exhibiting in due relation to one another a second set of curves in which each curve would be for a constant pressure, and in which the co-ordinates would represent temperatures and corresponding volumes. It serves generally as an aid towards bringing the whole subject clearly before the mind."

the process would lead to the conjecture that liquefaction had actually taken place, although optical tests carefully applied failed at any time to discover the presence of a liquid in contact with a gas. But against this view it may be urged with great force, that the fact of additional pressure being always required for a further diminution of volume, is opposed to the known laws which hold in the change of bodies from the gaseous to the liquid state. Besides, the higher the temperature at which the gas is compressed, the less the fall becomes, and at last it disappears.

The answer to the foregoing question, according to what appears to me to be the true interpretation of the experiments already described, is to be found in the close and intimate relations which subsist between the gaseous and liquid states of matter. The ordinary gaseous and ordinary liquid states are, in short, only widely separated forms of the same condition of matter, and may be made to pass into one another by a series of gradations so gentle that the passage shall nowhere present any interruption or breach of continuity. From carbonic acid as a perfect gas to carbonic acid as a perfect liquid, the transition we have seen may be accomplished by a continuous process, and the gas and liquid are only distant stages of a long series of continuous physical changes. Under certain conditions of temperature and pressure, carbonic acid finds itself, it is true, in what may be described as a state of instability, and suddenly passes, with evolution of heat, and without application of additional pressure or change of temperature, to the volume, which by the continuous process can only be reached through a long and circuitous route. In the abrupt change which here occurs, a marked difference is exhibited, while the process is going on, in the optical and other physical properties of the carbonic acid which has collapsed into the smaller volume, and of the carbonic acid not yet altered. There is no difficulty here, therefore, in distinguishing between the liquid and the gas. But in other cases the distinction cannot be made; and under many of the conditions I have described it would be vain to attempt to assign carbonic acid to the liquid rather than the gaseous state. Carbonic acid, at the temperature of 35°·5, and under a pressure of 108 atmospheres, is reduced to $\frac{1}{340}$ of the volume it occupied under a pressure of one atmosphere; but if anyone ask whether it is now in the gaseous or liquid state, the question does not, I believe, admit of a positive reply. Carbonic acid at 35°·5, and under 108 atmospheres of pressure, stands nearly midway between the gas and the liquid; and we have no valid grounds for assigning it to the one form of matter any more than to the other. The same observation would apply with even greater force to the state in which carbonic acid exists at higher temperatures and under greater pressures than those just mentioned. In short, the passage under great pressures from the liquid to the gaseous state may be effected by the application of heat without break or breach of continuity. That a marked change in the physical properties of the substance occurs during this process is no objection to its being con-

tinuous. . If mercury as a liquid is opaque and as a gas is transparent, the red and translucent bromine, on the other hand, when heated above the critical point, becomes so opaque as almost to resemble a mass of resin. Frankland has shown that the flame of hydrogen becomes continuous when the gas is burned under a pressure of 20 atmospheres, and these experiments have been since extended by the same able chemist and Lockyer. We must not, however, suppose that one intermediate state exists between liquid and gas; on the contrary, an infinite succession of intermediate states may truly be said to connect the liquid proper and the gas proper; in other words, the passage is continuous. When the critical point is attained, the density of the liquid and gas becomes the same, and the tube is filled with homogeneous matter.

As regards the question of the continuity of the solid and liquid states, it would be necessary, in order to establish this continuity, to obtain, by the combined action of heat and pressure, the solid and liquid of the same density and of like physical properties. To accomplish this result will probably require pressures far beyond any which can be reached in transparent tubes; but it may be possible to show by experiment that the solid and liquid can be made to approach to the required conditions.

[T. A.]

GENERAL MONTHLY MEETING,

Monday, June 5, 1871.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Silas Kemball Cook, Esq.
Miss Elinor Martin,
Charles Bland Radcliffe, M.D.
Mrs. Radcliffe,

were *elected* Members of the Royal Institution.

The special thanks of the Members were returned for the following Donation to "The Fund for the Promotion of Experimental Researches":—

Sir Henry Holland, Bart. (*13th Annual Donation*) £40

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Asiatic Society of Bengal—Journal, No. 160. 8vo. 1871.
Astronomical Society, Royal—Proceedings, Vol. XXXI. No. 6. 1871.
British Architects, Royal Institute of—Sessional Papers, 1870–71. No. 9. 4to.
Chemical Society—Journal for May, 1871. 8vo.

Editors—Academy for May, 1871. 4to.
American Journal of Science, April, 1871. 8vo.
Artizan for May, 1871. 4to.
Athenæum for May, 1871. 4to.
Chemical News for May, 1871. 4to.
Engineer for May, 1871. fol.
Food Journal for May, 1871. 8vo.
Horological Journal for May, 1871. 8vo.
Journal of Gas-Lighting for May, 1871. 4to.
Mechanics' Magazine for May, 1871. 8vo.
Nature for May, 1871. 4to.
Pharmaceutical Journal for May, 1871. 8vo.
Photographic News for May, 1871. 4to.
Franklin Institute—Journal, Nos. 542, 543. 8vo. 1871.
Medical and Chirurgical Society, Royal—Proceedings, Part 32. 8vo. 1871.
Macpherson, John, M.D. M.R.I. (the Author)—Our Baths and Wells. 16mo. 1871.
Markham, Clements R. Esq. (the Author)—Memoir on Indian Surveys. 8vo. 1871.
Murchison, Sir Roderick I., K.C.B. M.R.I. (the Author)—Address to the Royal Geographical Society, May 22, 1871. 8vo. 1871.
Photographic Society—Journal, No. 225. 8vo. 1871.
Preussische Akademie der Wissenschaften—Monatsberichte, April, 1871. 8vo.
Verzeichniss der Abhandlungen, 1710–1870. 8vo. 1871.
Royal Society of Tasmania—Proceedings, 1868, 1869. 8vo. 1869–70.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, Jan.–Feb. 1871.
Wilkinson, F. C. Esq.—The Gospel according to St. Mark (translated by J. Ivatt Briscoe). 4to. 1870.
Zoological Society of London—Transactions, Vol. VII. Parts 3, 4, 5. 4to. 1871.
Proceedings for 1870. 8vo. 1871.

WEEKLY EVENING MEETING,

Friday, June 9, 1871.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

John Tyndall, Esq. LL.D. F.R.S.

PROFESSOR OF NATURAL PHILOSOPHY, ROYAL INSTITUTION,

On Dust and Smoke.

After a few preliminary experiments illustrative of the polarization of light, Professor Tyndall adverted to the polarization of light by fine dust, by the sky, and by the coarser particles of smoke. In the former the direction of maximum polarization, as in the case of the sky, is at right angles to the illuminating beam. In the latter, according to the observations of Govi, the maximum quantity of polarized light was discharged obliquely to the beam. Govi's observation of a neutral point in such beam, on one side of which the polarization was positive and on the other side negative, was also

referred to. The additional fact was then adduced that the position of the neutral point varied with the density of the smoke. Beginning, for example, with an atmosphere thickened by the dense fumes of incense, resin, or gunpowder, and observing the neutral point, its direction was first observed to be inclined to the beam *towards* the source of illumination. Opening the windows so as to allow the smoke to escape gradually, the neutral point moved down the beam, passed the end of a normal drawn to the beam from the eye, and gradually moved forward several feet down the beam. The speaker did not halt at these observations; they were introduced as the starting-point of inquiries of a different nature, and after their introduction the discourse proceeded thus:—

But what, you may ask, is the practical good of these curiosities? And if you so ask, my object is in some senses gained, for I intended to provoke this question. I confess that if we exclude the interest attached to the observation of new facts, and the enchancement of that interest through the knowledge that by-and-by the facts will become the exponents of laws, these curiosities are in themselves worth nothing. They will not enable us to add to our stock of food, or drink, or clothes, or jewellery. But though thus shorn of all usefulness in themselves, they may, by leading the mind into places which it would not otherwise have entered, become the antecedents of practical consequences. In looking, for example, at this illuminated dust, we may ask ourselves what it is. How does it act, not upon a beam of light, but upon our own lungs and stomachs? The question at once assumes a practical character. We find on examination that this dust is organic matter—in part living, in part dead. There are among it particles of ground straw, torn rags, smoke, the pollen of flowers, the spores of fungi, and the germs of other things. But what have they to do with the animal economy? Let me give you an illustration to which my attention has been lately drawn by Mr. George Henry Lewes, who writes to me thus:—

"I wish to direct your attention to the experiments of Von Recklingshausen should you happen not to know them. They are striking confirmations of what you say of dust and disease. Last spring, when I was at his laboratory in Würzburg, I examined with him blood that had been three weeks, a month, and five weeks, out of the body, preserved in little porcelain cups under glass shades. This blood was living and growing. Not only were the Amœba-like movements of the white corpuscles present, but there were abundant evidences of the growth and development of the corpuscles. I also saw a frog's heart still pulsating which had been removed from the body (I forget how many days, but certainly more than a week). There were other examples of the same persistent vitality or absence of putrefaction. Von Recklingshausen did not attribute this to the absence of germs—germs were not mentioned by him; but when I asked him how he represented the thing to himself, he said the whole mystery of his operation consisted in keeping the blood *free from dirt*.

The instruments employed were raised to a red heat just before use, the thread was silver thread and was similarly treated, and the porcelain cups, though not kept free from air, were kept free from currents. He said he often had failures, and these he attributed to particles of dust having escaped his precautions."

Professor Lister, who has founded upon the removal or destruction of this "dirt" great and numerous improvements in surgery, tells us the effect of its introduction into the blood of wounds. He informs us what would happen with the extracted blood should the dust get at it. The blood would putrefy and become fetid; and when you examine more closely what putrefaction means, you find the putrefying substance swarming with organic life, the germs of which have been derived from the air.

Another note which I received a day or two ago has a bearing particularly significant at the present time upon this question of dust and dirt, and the wisdom of avoiding them. The note is from Mr. Ellis, of Sloane Street, to whom I owe a debt of gratitude for advice given to me when sorely wounded in the Alps. "I do not know," writes Mr. Ellis, "whether you happened to see the letters, of which I enclose you a reprint, when they appeared in 'The Times.' But I want to tell you this in reference to my method of vaccination as here described; because it has, as I think, a relation to the subject of the intake of organic particles from without into the body. Vaccination in the common way is done by scraping off the epidermis, and thrusting into the punctures made by the lancet the vaccine virus. By the method I use (and have used for more than twenty years) the epidermis is lifted by the effusion of serum from below, a result of the irritant cantharadine applied to the skin. The little bleb thus formed is pricked, a drop of fluid let out, and then a fine vaccine point is put into this spot, and after a minute of delay it is withdrawn. The epidermis falls back on the skin, and quite excludes the air—and not the air only, but what the air contains.

"Now mark the result—out of hundreds of cases of re-vaccination which I have performed, I have never had a single case of blood-poisoning or of abscess. By the ordinary way the occurrence of secondary abscess is by no means uncommon, and that of pyæmia is occasionally observed. I attribute the comparative safety of my method entirely, first, to the exclusion of the air and what it contains; and, secondly, to the greater size of the apertures for the inlet of mischief made by the lancet."

I bring these facts forward that they may be sifted and challenged if they be not correct. If they are correct it is needless to dwell upon their importance, nor is it necessary to say that if Mr. Ellis had resigned himself wholly to the guidance of the germ theory he could not have acted more in accordance with the requirements of that theory than he has actually done. It is what the air contains that does the mischief in vaccination. Mr. Ellis's results fall in with the general theory of putrefaction propounded by Schwann, and developed in this

country with such striking success by Professor Lister. They point, if true, to a cause distinct from bad lymph for the failures and occasional mischief incidental to vaccination; and if followed up they may be the means of leaving the irrational opposition to vaccination no ground to stand upon, by removing even the isolated cases of injury on which the opponents of the practice rely.

We are now assuredly in the midst of practical matters. With your permission I will recur once more to a question which has recently occupied a good deal of public attention. You know that as regards the lowest forms of life, the world is divided, and has for a long time been divided, into two parties, the one affirming that you have only to submit absolutely dead matter to certain physical conditions to evolve from it living things; the other, without wishing to set bounds to the power of matter, affirming that in our day no life has ever been found to arise independently of pre-existing life. Many of you are aware that I belong to the party which claims life as a derivative of life. The question has two factors: the evidence, and the mind that judges of the evidence; and you will not forget that it may be purely a mental set or bias on my part that causes me throughout this discussion from beginning to end, to see on the one side dubious facts and defective logic, and on the other side firm reasoning and a knowledge of what rigid experimental inquiry demands. But judged of practically, what, again, has the question of Spontaneous Generation to do with us? Let us see. There are numerous diseases of men and animals that are demonstrably the products of parasitic life, and such diseases may take the most terrible epidemic forms, as in the case of the silkworms of France in our day. Now it is in the highest degree important to know whether the parasites in question are spontaneously developed, or are wafted from without to those afflicted with the disease. The means of prevention, if not of cure, would be widely different in the two cases.

But this is by no means all. Besides these universally admitted cases, there is the broad theory now broached and daily growing in strength and clearness—daily, indeed, gaining more and more of assent from the most successful workers and profound thinkers of the medical profession itself—the theory, namely, that contagious disease generally is of this parasitic character. If I had heard or read anything since to cause me to regret having introduced this theory to your notice more than a year ago, I should here frankly express that regret. I would renounce in your presence whatever leaning towards the germ theory my words might then have betrayed. Let me state in two sentences the grounds on which the supporters of the theory rely. From their respective viruses you may plant typhoid fever, scarlatina, or small-pox. What is the crop that arises from this husbandry? As surely as a thistle rises from a thistle seed, as surely as the fig comes from the fig, the grape from the grape, the thorn from the thorn, so surely does the typhoid virus increase and multiply into typhoid fever, the scarlatina virus into scarlatina, the small-pox

virus into small-pox. What is the conclusion that suggests itself here? It is this:—That the thing which we vaguely call a virus is to all intents and purposes a *seed:* that, excluding the notion of vitality, in the whole range of chemical science you cannot point to an action which illustrates this perfect parallelism with the phenomena of life—this demonstrated power of self-multiplication and reproduction. The germ theory alone accounts for the phenomena.

And here you see the bearing of the doctrine of Spontaneous Generation upon this question. For if the doctrine continues to be discredited as it has hitherto been, it will follow that the epidemics which spread havoc amongst us from time to time are not spontaneously generated, but that they arise from an ancestral stock whose habitat is the human body itself. It is not on bad air or foul drains that the attention of the physician will primarily be fixed, but upon disease germs which no bad air or foul drains can create, but which may be pushed by foul air into virulent energy of reproduction. You may think I am treading on dangerous ground, that I am putting forth views that may interfere with salutary practice. No such thing. If you wish to learn the impotence of medical science and practice in dealing with contagious diseases, you have only to refer to a recent Harveian oration by Dr. Gull. Such diseases defy the physician. They must burn themselves out. And indeed this, though I do not specially insist upon it, would favour the idea of their vital origin. For if the seeds of contagious disease be themselves living things, it will be difficult to destroy either them or their progeny without involving their living habitat in the same destruction.

And I would also ask you to be cautious in accepting the statement which has been so often made, and which is sure to be repeated, that I am quitting my own *métier* when I speak of these things. I am not dealing with professional questions. I am writing no prescription, nor should I venture to draw any conclusion from the condition of your pulse and tongue. I am dealing with a question on which minds accustomed to weigh the value of experimental evidence are alone competent to decide, and regarding which, in its present condition, minds so trained are as capable of forming an opinion as on the phenomena of magnetism and radiant heat. I cannot better conclude this portion of my story than by reading to you an extract from a letter addressed to me some time ago by Dr. William Budd, of Clifton, to whose insight and energy the town of Bristol owes so much in the way of sanitary improvement.

"As to the germ theory itself," writes Dr. Budd, "that is a matter on which I have long since made up my mind. From the day when I first began to think of these subjects, I have never had a doubt that the specific cause of contagious fevers must be living organisms.

"It is impossible, in fact, to make any statement bearing upon the essence or distinctive characters of these fevers, without using terms which are of all others *the most distinctive of life*. Take up the

writings of the most violent opponent of the germ theory, and, ten to one, you will find them full of such terms as 'propagation,' 'self-propagation,' 'reproduction,' 'self-multiplication,' and so on. Try as he may—if he has anything to say of those diseases which is characteristic of them—he cannot evade the use of these terms, or the exact equivalents to them. While perfectly applicable to living things, these terms express qualities which are not only inapplicable to common chemical agents, but as far as I can see actually inconceivable of them."

Once then established within the body, this evil form of life, if you will allow me to call it so, must run its course. Medicine as yet is powerless to arrest its progress, and the great point to be aimed at is to prevent its access to the body. It was with this thought in my mind that I ventured to recommend, more than a year ago, the use of cotton-wool respirators in infectious places. I would here repeat my belief in their efficacy if properly constructed. But I do not wish to prejudice the use of these respirators in the minds of its opponents by connecting them indissolubly with the germ theory. There are too many trades in England where life is shortened and rendered miserable by the introduction of matters into the lungs which might be kept out of them. Dr. Greenhow has shown the stony grit deposited in the lungs of stonecutters. The black lungs of colliers is another case in point. In fact, a hundred obvious cases might be cited, and others that are not obvious might be added to them. We should not, for example, think that printing implied labours where the use of cotton-wool respirators might come into play; but I am told that the dust arising from the sorting of the type is very destructive of health. I went some time ago into a manufactory in one of our large towns, where iron vessels are enamelled by coating them with a mineral powder, and subjecting them to a heat sufficient to fuse the powder. The organization of the establishment was excellent, and one thing only was needed to make it faultless. In a large room a number of women were engaged covering the vessels. The air was laden with the fine dust, and their faces appeared as white and bloodless as the powder with which they worked. By the use of cotton-wool respirators these women might be caused to breathe air more free from suspended matters than that of the open street. Over a year ago I was written to by a Lancashire seedsman, who stated that during the seed season of each year his men suffered horribly from irritation and fever, so that many of them left his service. He asked me could I help him, and I gave him my advice. At the conclusion of the season this year he wrote to me that he had simply folded a little cotton-wool in muslin, and tied it in front of the mouth; that he had passed through the season in comfort and without a single complaint from one of his men.

The substance has also been turned to other uses. An invalid tells me that at night he places a little of the wool before his mouth, slightly moistening it to make it adhere; that he has thereby pro-

longed his sleep, abated the irritation of his throat, and greatly mitigated a hacking cough from which he had long suffered. In fact, there is no doubt that this substance is capable of manifold useful applications. An objection was urged against the use of it: that it became wet and heated by the breath. While I was casting about for a remedy for this, a friend forwarded to me from Newcastle a form of respirator invented by Mr. Carrick, an hotel-keeper at Glasgow, which meets the case effectually, and by a slight modification may be caused to meet it perfectly. The respirator, with its back in part removed, is shown in Fig. 1. It consists of the space under the partition of wire gauze *q r*, intended by Mr. Carrick for "medicated substances," and which may be filled with cotton-wool. The mouth is placed against the aperture O, which fits closely round the lips, and the air enters the mouth through the cotton-wool by a light valve V, which is lifted by the act of inhalation. During exhalation this valve closes; another breath escapes by a second valve, V′, into the open air. The wool is thus kept dry and cool; the air in passing through it being filtered of everything it holds in suspension.*

Fig. 1.

We have thus been led by our first unpractical experiments into a thicket of practical considerations. In taking the next step, a personal peculiarity had some influence upon me. The only kind of fighting in which I take the least delight, is the conflict of man with nature. I like to see a man conquer a peak or quench a conflagration. I remember clearly the interest I took twenty years ago in seeing the firemen of Berlin contending for mastery with a fire which had burst somewhere near the Brandenburger Thor; and I have often experienced the same interest in the streets of London. Admiring as I do the energy and bravery of our firemen, and having heard that smoke was a greater enemy to them than flame itself, the desire arose of devising a fireman's respirator. But before I describe what has been done in this direction, let me draw your attention to the means hitherto employed to enable a man to live in dense smoke. Thanks to the courtesy of Capt. Shaw, I am enabled to show you the action of the "smoke-jacket," known abroad as the "Appareil Paulin," from its supposed inventor. The jacket is of pliable cowhide. It has arms and a hood, with eye-glasses. With straps and buckles the jacket is tied round the wrists and waist, and a strap which passes

* Mr. Ladd, of Beak Street, sells these respirators.

between the legs prevents it from rising. On the left side of the jacket is fixed a screw, to which the ordinary hose of the fire-engine is attached, and through the hose air instead of water is urged into the space between the fireman's body and the jacket. It becomes partially inflated, but no pressure of any amount is attainable, because the air, though somewhat retarded, escapes with tolerable freedom from the wrists and waist. Hence the fireman, when his hose is long enough, can deliberately walk into the densest smoke or foulest air. But you see the use of the smoke-jacket necessitates the presence of several men; it also implies the presence of an engine. A single man could make no use of it, nor indeed any number of men without a pumping engine. Its uses are thus summed up in a communication addressed to me by Captain Shaw:—

"This smoke-jacket is very useful for extinguishing fires in vaults, stopping conflagrations in the holds of ships, and penetrating wells, quarries, mines, cesspools, &c.—any places, in short, where the air has become unfit for respiration.

"The special advantages of this jacket are its great simplicity, its facility for use, and the rapidity with which it can be carried about and put on; but its drawback is that it requires the use of an engine or air-pump, and consequently is of no service to one man alone. For this latter reason smoke-jackets, although very effective for enabling us to get into convenient places for extinguishing fires, have very rarely proved of any avail for *saving life*."

Now, it is that very want that I thought ought to be supplied by a suitable respirator. Our fire-escapes are each in charge of a single man, and I wished to be able to place it in the power of each of those men to penetrate through the densest smoke into the recesses of a house, and there to rescue those who would otherwise be suffocated or burnt. I thought that cotton-wool, which so effectually arrested dust, might also be influential in arresting smoke. It was tried; but, though found soothing in certain gentle kinds of smoke, it was no match for the pungent fumes of a resinous fire, which we employ in our experiments in the laboratory, and which, I am gratified to learn from Captain Shaw, evolves the most abominable smoke with which he is acquainted. I cast about for an improvement, and in conversing on the subject with my friend Dr. Debus, he suggested the use of glycerine to moisten the wool, and render it more adhesive. In fact, this very substance had been employed by the most distinguished advocate of the doctrine of spontaneous generation, M. Pouchet, for the purpose of catching the atmospheric germs. He spread a film of glycerine on a plate of glass, urged air against the film, and examined the dust which stuck to it. The moistening of the cotton-wool with this substance was a decided improvement; still the respirator only enabled us to remain in dense smoke for three or four minutes, after which the irritation became unendurable.* Reflection suggested that in combus-

* The nose, with its hairs and mucus, acts, though imperfectly, the part of the moistened cotton wool respirator.

tion so imperfect as the production of dense smoke implies, there must be numerous hydrocarbons produced, which, being in a state of vapour, would be very imperfectly arrested by the cotton-wool. These in all probability were the cause of the residual irritation; and if these could be removed, a practically perfect respirator might possibly be obtained.

I state the reasoning exactly as it occurred to my mind. Its result will be anticipated by many present. All bodies possess the power of condensing in a greater or less degree gases and vapours upon their surfaces, and when the condensing body is very porous, or in a fine state of division, the force of condensation may produce very remarkable effects. Thus, a clean piece of platinum-foil placed in a mixture of oxygen and hydrogen so squeezes the gases together as to cause them to combine; and if the experiment be made with care, the heat of combination may raise the platinum to bright redness, so as to cause the remainder of the mixture to explode. The promptness of this action is greatly augmented by reducing the platinum to a state of fine division. A pellet of "spongy platinum," for instance, plunged into a mixture of oxygen and hydrogen, causes the gases to explode instantly. In virtue of its extreme porosity, a similar power is possessed by charcoal. It is not strong enough to cause the oxygen and hydrogen to combine like the spongy platinum, but it so squeezes the more condensable vapours, and also acts with such condensing power upon the oxygen of the air, as to bring both within the combining distance, thus enabling the oxygen to attack and destroy the vapours in the pores of the charcoal. In this way, effluvia of all kinds may be virtually burnt up; and this is the principle of the excellent charcoal respirators invented by Dr. Stenhouse. Armed with one of these, you may go into the foulest-smelling places without having your nose offended. Some of you will remember Dr. Stenhouse lecturing in this room with a suspicious-looking vessel in front of the table. That vessel contained a decomposing cat. It was covered with a layer of charcoal, and nobody knew until told of it what the vessel contained.*

I may be permitted in passing to give my testimony as to the efficacy of these charcoal respirators in providing warm air for the lungs. Not only is the sensible heat of the breath in part absorbed by the charcoal, but the considerable amount of latent heat which accompanies the aqueous vapour from the lungs is rendered free by the condensation of the vapour in the pores of the charcoal. Each particle of charcoal is thus converted into an incipient ember, and warms the air as it passes inwards. This is in entire accordance with the thermometric observations of Dr. Marcet.

But while powerful to arrest vapours, the charcoal respirator is ineffectual as regards smoke. The particles get freely through the

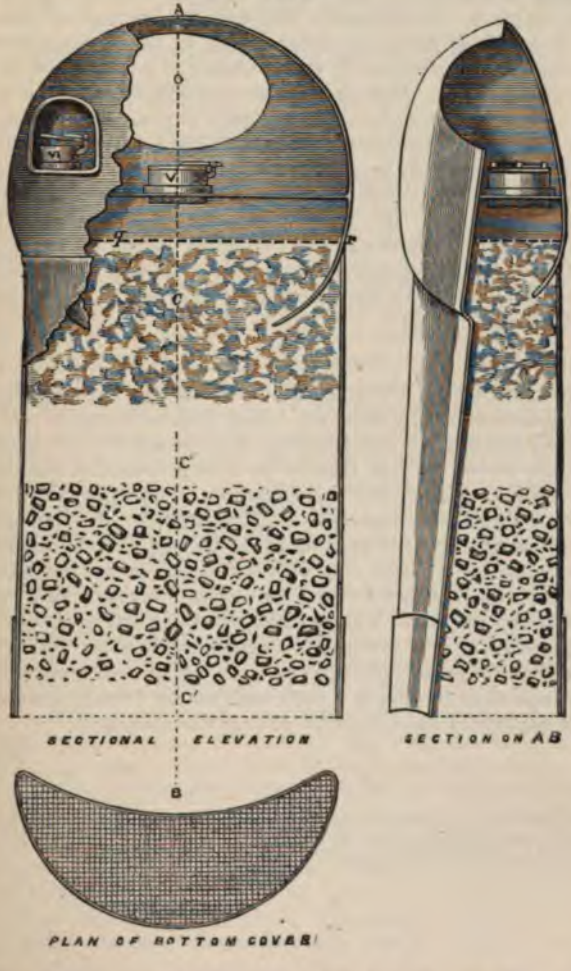
* Dr. Stenhouse has recently shown me a respirator, invented by him many years ago, in which, by means of a single valve, the inhaled air passes through charcoal to the lungs, while that exhaled escapes, without passing through the charcoal, into the open air.

respirator. In a series of them tested downstairs, from half a minute to a minute was the limit of endurance. This might be exceeded by Faraday's method of emptying the lungs completely, and then filling them before going into a smoky atmosphere. In fact, each solid smoke particle is itself a bit of charcoal, and carries on it, and in it, its little load of irritating vapour. It is this, far more than the particles of carbon themselves, that produces the irritation. Hence two causes of offence are to be removed: the carbon particles which convey the irritant by adhesion and condensation, and the free vapour which accompanies the particles. The moistened cotton-wool I knew would arrest the first, fragments of charcoal I hoped would stop the second. In the first fireman's respirator, Mr. Carrick's arrangement of two valves, the one for inhalation, the other for exhalation, are preserved. But the portion of it which holds the filtering and absorbent substances is prolonged to a depth of four or five inches (see Fig. 2). On the partition of wire gauze qr at the bottom of the space which fronts the mouth is placed a layer of cotton-wool, C, moistened with glycerine; then a thin layer of dry wool, C'; then a layer of charcoal fragments; a second thin layer of dry cotton-wool, succeeded by a layer of fragments of caustic lime. The succession of the layers may be changed without prejudice to the action. A wire-gauze cover, shown in plan below Fig. 2, keeps the substances from falling out of the respirator. In the densest smoke that we have hitherto employed, the layer of lime has not been found necessary, nor is it shown in the figure; in a flaming building, indeed, the mixture of air with the smoke never permits the carbonic acid to become so dense as to be irrespirable; but in a place where the gas is present in undue quantity, the fragments of lime would materially mitigate its action.

In a small cellar-like chamber downstairs, with a stone flooring and stone walls, the first experiments were made. We placed there furnaces containing resinous pine-wood, lighted the wood, and placing over it a lid which prevented too brisk a circulation of the air, generated dense volumes of smoke. With our eyes protected by suitable glasses, my assistant and I have remained in this room for half an hour and more, when the smoke was so dense and pungent that a single inhalation through the undefended mouth would be perfectly unendurable: and we might have prolonged our stay for hours. Having thus far perfected the instrument, I wrote to Captain Shaw, the chief officer of the Metropolitan Fire Brigade, asking him whether such a respirator would be of use to him. His reply was prompt; it would be most valuable. He had, however, made himself acquainted with every contrivance of the kind in this and other countries, and had found none of them of any practical use. He offered to come and test it here, or to place a room at my disposal in the City. At my request he came here, accompanied by three of his men. Our small room was filled with smoke to their entire satisfaction. The three men went successively into it, and remained there as long as Captain Shaw wished them. On coming out they said that they had not suffered the slightest inconvenience; that

they could have remained all day in the smoke. Captain Shaw then tested the instrument with the same result. From that hour the greatest interest has been taken in the perfecting of the instrument

Fig. 2.



by Captain Shaw himself. He has attached to the respirator suitable hoods. The real problem is practically solved, and I can only say that if a tithe of the zeal, intelligence, and practical skill were bestowed on the cotton-wool respirator that Captain Shaw has devoted to the fire-

man's respirator the sufferings of many a precious life might be spared and its length augmented.*

The discourse was concluded as follows:—"Thus have we been led from the actinic decomposition of vapours through the tails of comets and the blue of the sky to the dust of London, from the germ theory of disease down to this fireman's respirator. Instead of this trivial example, I could, if time permitted, point to others of a more considerable kind in illustration of the tendency of pure science to lead to practical applications. Indeed those very wanderings of the scientific intellect which at first sight appear utterly unpractical, become in the end the wellsprings of practice. Yet I believe there is a philosophy embraced by some of our more ardent thinkers (who I fear on many points commit the well-intentioned, but fatal mistake of putting their own hopeful fancies in the place of fact) that would abolish these wanderings of the intellect and fix it from the outset on practical ends alone. I do not think that that philosophy will ever make itself good in the world, or that any freedom-loving student of nature could or would tolerate its chains."

A short time before the discourse I had an opportunity of inspecting the apparatus of Mr. Sinclair, which has been tested and highly spoken of by the superintendent of the Manchester Fire Brigade. The original idea is due to Von Humboldt, who proposed it for the Hartz mines. Galibert constructed the apparatus in an improved form, and it has been still further improved by Mr. Sinclair, who has purchased Galibert's patent. It consists of an air-tight bag, from which issue two tubes that unite on a single one with a respirator mouth-piece. The bag is filled with air, and the wearer inspires through one valve and expires through another. The expired breath is carried to the bottom of the bag, and is stated to remain there in consequence of the chilling experienced in its passage downwards. A bag of not inordinate size is said to be sufficient to supply a man with air for twenty minutes. Mr. Sinclair's apparatus was exhibited during the discourse.

[J. T.]

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* Mr. Ladd has also proposed a form of mouth-piece which promises well, and Mr. Cottrell has attached to it an ordinary fencing-mask. This will probably be the form of apparatus finally adopted.

EXTRA EVENING MEETING,

Friday, June 16, 1871.

THE LORD LINDSAY, Vice-President, in the Chair.

WILLIAM BRADFORD, Esq.

On the Esquimaux and Ice of Greenland.

MR. BRADFORD gave an account of an Arctic expedition in which he was engaged in the autumn of 1869, illustrated by about seventy photographs, magnified and thrown upon the screen by means of the camera and electric lamp.

He began by alluding to the influence which the exuberant energies and awful manifestations of nature on the western continent had had upon the American school of landscape-painters, inducing them to choose for their subjects the greatest cataracts, the highest mountain chains, and similar stupendous phenomena, as represented in the works of Church and Bierstadt. Like these painters, Mr. Bradford said that he had been impelled to study nature in some of her most vast and strange aspects; and, being a native of New Bedford, in Massachusetts (a seat of the whale fishery), his imagination was naturally directed northward. After several times engaging a sailing-vessel for a four months' voyage to the coast of Labrador, in order to study the scenery of the northern seas, he was enabled, by the liberal aid of his countryman, Mr. Legrand Lockwood, of New York, to fit out a steamer in order to go as far north as possible. His friend Dr. J. J. Hayes consented to command the expedition, which sailed from Newfoundland on July 3, 1869, in an English steamer expressly built for Arctic navigation, the 'Panther,' Captain John Bartlett.

Mr. Bradford commenced his narrative by referring to his illustrations, representing the dangers of the navigation met with in approaching the coast of Greenland, through the great masses of field and hummock ice and icebergs brought down by the great Spitzbergen current. After sailing many miles in calm water the vessel encountered a heavy gale, from which it escaped by weathering under the lee of an iceberg. From Cape Desolation, a barren rock about 2000 feet high, they made their way south to Julianeshab, one of the largest and most thriving settlements in South Greenland, inhabited by about 300 Esquimaux, many of whom are half Danes in blood and habits. Numerous illustrations were given of these people, and their costumes, houses, boats, &c.; and among the representations of the relics of the ancient Northmen were the remains of an old church or cathedral, at the head of Erick's Fiord, about fifteen miles from Julianeshab. The walls of this church and some of the houses are still in a good state of preservation, although the date of the settlement according to

Iceland began in about 900—1000 A.D. Fragments of a wall which surrounded the whole of the buildings still remain.

Passing up the coast, they came to the great Sermitsialik glacier, about three miles wide, in which is seen, when coming up the fiord, what appears to be a bridge of silver-like crystal, connecting it with the shore, in front rising perpendicularly from 250 feet to 275 feet above the water, and extending downwards 500 feet beneath the surface. This glacier rises above the highest mountains, and loses itself finally in the great Mer de Glace, in the unknown interior. Large caverns and tunnels have been formed in the glacier by the action of currents of water, which carry down immense quantities of sand, and it once forced itself down the fiord, ploughing up a moraine of earth above 30 feet high, and throwing out huge boulders of rock. The party witnessed the rare sight of a stupendous discharge from the glacier front, giving birth to eight huge icebergs, with several smaller ones: and the debris so filled and packed the waters of the fiord in a few minutes, that it was almost impossible to get a boat 10 ft. long through the mass.

Leaving the glaciers, the expedition passed on to the island of Disco, and entered the harbour of Godhaven, the rendezvous of the whalers sailing northward. Here they were hospitably entertained by the Inspector, Mr. Smith, and the Governor, Hansen, who had shown great kindness to Dr. Kane, Dr. Hayes, and others. The party soon after entered the awfully magnificent scenery of Karsut Fiord, with perpendicular cliffs, between three and four thousand feet high. About fifteen miles from this, Upernavik is the most northern settlement in the world, peopled by about two hundred Esquimaux, ruled by the hospitable Governor Rudolph.

The expedition then proceeded to Melville Bay, the terror of Arctic voyagers, the scenery of which was illustrated by numerous photographs, exhibiting the passage of the steamer through the fields of ice; the landmark termed "The Devil's Thumb," rising about 1200 feet above the plateau of a mountain; the beautiful opalescent appearance of the ice produced by the midnight sun; polar bears in search of food, &c.

[In the library were placed several hundred photographs showing the varied appearances of the ice and icebergs, and the Esquimaux and their houses; together with several oil-paintings, the largest being "Summer in the Arctic Regions, or Boring through the Pack in Melville Bay, August, 1869."]

[W. B.]

GENERAL MONTHLY MEETING,

Monday, July 3, 1871.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

William Amhurst Tyssen Amhurst, Esq. F.S.A. M.R.S.L.
Lawrence Trent Cave, Esq. F.R.G.S.

were *elected* Members of the Royal Institution.

The special thanks of the Members were returned to the Lord Lindsay for his present of a valuable spectroscope, employed by Dr. Huggins in his researches into the Physical and Chemical Constitution of the Stars and Nebulæ.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Spanish Government—Estadistica Minera correspondiente al año de 1868. 4to. 1870.
Memoria sobre el Ensayo comparativo del Sistema de Distilacion de los Minerales de Mercurio proposto por M. Emile Pellet, &c. 4to. 1871.
Academy of Natural Sciences, Philadelphia—Proceedings. 1870. 8vo.
Astronomical Society, Royal—Proceedings, Vol. XXXI. No. 7. 1871.
Author—Sketches of the House of Commons. By a Silent Member. (() 14) 16mo. 1871.
Chemical Society—Journal for June, 1871. 8vo.
Comitato Geologico d'Italia—Bollettini. 1871. No. 3, 4. 8vo. Firenze.
Cornwall Polytechnic Society, Royal—Thirty-third, thirty-fifth, thirty-seventh, and thirty-eighth Reports. 8vo. 1865–71.
Editors—Academy for June, 1871. 4to.
American Journal of Science for May and June, 1871. 8vo.
Artizan for June, 1871. 4to.
Athenæum for June, 1871. 4to.
Chemical News for June, 1871. 4to.
Engineer for June, 1871. fol.
Horological Journal for June, 1871. 8vo.
Journal of Gas-Lighting for June, 1871. 4to.
Mechanics' Magazine for June, 1871. 8vo.
Nature for June, 1871. 4to.
Pharmaceutical Journal for June, 1871. 8vo.
Photographic News for June, 1871. 4to.
Scientific Review for June, 1871. fol.
Ellis, George E. Esq. (the Author)—Memoir of Sir Benjamin Thompson, Count Rumford, with Notices of his Daughter. 8vo. 1871.

Franklin Institute—Journal, No. 544. 8vo. 1871.
Linnean Society—Journal, Nos. 51, 56. 8vo. 1871.
Medical and Chirurgical Society, Royal—General Index to the Transactions. Vol. I.–LIII. 8vo. 1871.
Meteorological Society—Proceedings, No. . 8vo. 1871.
Photographic Society—Journal, No. 226. 8vo. 1871.
Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, June, 1871. 8vo.
Tyndall, John, Esq. LL.D. F.R.S. (the Author)—Hours of Exercise in the Alps. 16mo. 1871.
United Service Institution, Royal—Journal, No. 62. 8vo. 1871.

Royal Institution of Great Britain.

GENERAL MONTHLY MEETING,

Monday, Nov. 6, 1871.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Sir John Conroy, Bart.
Dr. J. Hall Davis.
Ralph Henry Christopher Nevile, Esq.
Henry Rigg, Esq.

were *elected* Members of the Royal Institution.

The Managers reported that they had reappointed WILLIAM ODLING, M.B. F.R.S. Fullerian Professor of Chemistry on July 10 last.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

Governor-General of India—Memoirs of the Geological Survey of India, Vol. VII. Parts 1, 2, 3. 8vo. 1869-70.
Records, Vol. II. 2-4; Vol. III.; Vol. IV. 1, 2. 8vo.
Palæontologia Indica, Series VI. Parts 1-8. fol. 1870-1.
The Lords of the Admiralty—The Nautical Almanack for 1875. 8vo. 1871.
Commissioners in Lunacy—25th Report. 8vo. 1871.
Actuaries, Institute of—Journal, Nos. 84, 85. 8vo. 1871.
Agricultural Society of England—Journal, New Series, No. 14. 8vo. 1871.
Airy, G. B. Esq. F.R.S. Astronomer Royal—Halley's Magnetic Chart.
American Academy of Arts and Sciences—Proceedings, Vol. VIII. Nos. 18-37. 8vo. 1869-70.
Complete Works of Count Rumford. Vol. I. 8vo. 1870.
American Philosophical Society—Proceedings, Nos. 83-85. 8vo. 1870.
Transactions, Vol. XIV. Part 1. 4to. 1870.
Antiquaries, Society of—Proceedings, Vol. V. No. 1. 8vo. 1871.
Asiatic Society of Bengal—Journal, No. 167. 8vo. 1871.
Proceedings, 1871. Nos. 3, 4, 5. 8vo.
Astronomical Society, Royal—Proceedings, Vol. XXXI. Nos. 8, 9. 1871.
Bartholomew's Hospital, St. (the Treasurer)—Reports, Vol. VII. 8vo. 1871.
Bavarian Academy of Sciences, Royal—Sitzungsberichte. 1870. II. Heft 3, 4. 1871; Heft 1. 8vo.
Baxter, R. D. Esq. M.R.I.—M. Nicolas, Études sur les Évangiles Apocryphes. 8vo. Paris, 1866.
M. Nicolas, Le Symbole des Apôtres. 8vo. Paris, 1867.
Belgique, Académie Royale des Sciences de—Bulletins, Tomes XXIX. XXX. 8vo. 1870.
Annuaire. 1871. 16to.
Annales Météorologiques de l'Observatoire de Belgique. 4me Année. 4to. 1870.

Best, Hon. and Rev. S. M.A. M.B.I. (*the Author*)—Sermons on the Beginning of All Things. 16to. 1871.

Boston Society of Natural History, U.S.—Memoirs, Vol. II. Part 11. 4to. 1870.
Proceedings, Vol. XIII. Nos. 15–23. 8vo. 1870.

British Association for the Advancement of Science—Report of the 40th Meeting, held at Liverpool in Sept. 1870. 8vo. 1871.

British Museum Trustees—Catalogus Codicum MSS. Orientalium, Pars 2, Codices Arabicos complectens. fol. 1871.
Hand List of Birds, Part 3. 8vo. 1871.
Catalogue of Dermaptera Saltatoria, Part 5. 8vo. 1871.
Guide to First Vase Room. 8vo. 1871.
Catalogue of Meteorites. 8vo. 1871.
Catalogue of Syriac MSS. Part 2. 4to. 1871.

Chemical Society—Journal for July, Aug. Sept. Oct. 1871. 8vo.

Clinical Society—Transactions, Vol. IV. 8vo. 1871.

Comitato Geologico d'Italia—Bollettini. 1871. Nos. 5, 6. 8vo. Firenze.

Connecticut Academy of Arts and Sciences—Transactions, Vol. II. 8vo. 1869–71.

Courtemanche, M. R. (*the Author*)—Description d'un Navire Aérien. (L 15) 8vo. Paris, 1871.

Cox, E. T. Esq. (*the Author*)—First Annual Report of the Geological Survey of Indiana. 8vo. 2 vols. 1869.

De la Rive, M. A. (*the Author*)—Notice sur E. Verdet. (L 15) 8vo. 1871.

Editors—Academy for July, Aug. Sept. Oct. 1871. 4to.
American Journal of Science for July, Aug. Sept. Oct. 1871. 8vo.
Artizan for July, Aug. Sept. Oct. 1871. 4to.
Athenæum for July, Aug. Sept. Oct. 1871. 4to.
Chemical News for July, Aug. Sept. Oct. 1871. 4to.
Engineer for July, Aug. Sept. Oct. 1871. fol.
Horological Journal for July, Aug. Sept. Oct. 1871. 8vo.
Journal of Gas-Lighting for July, Aug. Sept. Oct. 1871. 4to.
Mechanics' Magazine for July, Aug. Sept. Oct. 1871. 8vo.
Nature for July, Aug. Sept. Oct. 1871. 4to.
Pharmaceutical Journal for July, Aug. Sept. Oct. 1871. 8vo.
Photographic News for July, Aug. Sept. Oct. 1871. 4to.

Elliot, The Lady—Memoirs on the History, Folk Lore, and Distribution of the Races of the North-Western Provinces of India. By Sir H. M. Elliot. Edited by J. Beames. 2 vols. 8vo. 1869.

Essex Institute, U.S.—Proceedings, Vol. VI. Part 2. 1868–71. 8vo.
Bulletin, Vol. II. 8vo. 1870.
Greenwich Observations for 1869. 4to. 1871.

Franklin Institute—Journal, Nos. 545, 546, 547, 548, 549. 8vo. 1871.

Geographical Society, Royal—Proceedings, Vol. XV. Nos. 2, 3, 4. 8vo. 1871.
Journal, Vol. XL. 8vo. 1870.

Geological and Polytechnic Society of the West Riding of Yorkshire—Report of Proceedings for 1870. 8vo. 1871.

Geological Institute, Imperial, Vienna—Jahrbuch, 1871. No. 2. 8vo.
Verhandlungen, 1871. No. 1. 8vo.
Abhandlungen, Band V. Heft 1, 2. 4to. 1871.

Geological Society—Quarterly Journal, No. 107. 8vo. 1871.

Glasgow Philosophical Society—Proceedings, Vol. VII. Nos. 1–3. 8vo. 1868–71.

Harben, H. Esq. (*the Author*)—Mortality Experience of the Prudential Assurance Company, 1867–70. 8vo. 1871.

Institution of Civil Engineers—Minutes of Proceedings, Vols. XXXI. XXXII. and Index to Vols. XXI.–XXX. 8vo. 1871.

Kavanagh, Morgan, Esq. (*the Author*)—Origin of Language and Myths. 2 vols. 8vo. 1871.

Levittoux, M. H. (*the Author*)—Philosophie de la Nature. 8vo. Paris, 1871.

Linnean Society—Journal, Nos. 52, 53, 65. 8vo. 1871.
Proceedings, 1870–1. 8vo.
Transactions, Vol. XXVII. Part 3. 4to. 1871.
Liverpool Literary and Philosophical Society—Proceedings, Nos. 23, 24. 8vo. 1869–70.
Lubbock, Sir John, Bart. M.P. F.R.S. (the Author)—On the Development of Relationships, and on Stone Implements from Africa and Syria. (From Jour. Anthrop. Inst. I.) 8vo. 1871.
Mears, T. Lambert, Esq. LL.D. M.R.I.—Dr. G. B. Halford, On the New Treatment of Snake Bite. (K 98) 8vo. 1869.
Mechanical Engineers' Institution, Birmingham—Proceedings, April, 1871; July, Part 1. 8vo.
Medical and Chirurgical Society, Royal—Proceedings, Part 33. 8vo. 1871.
Meteorological Committee of the Royal Society—Report for 1870. 8vo. 1871.
Contributions to the Meteorology of Cape Horn. 4to. 1871.
Meteorological Office—Quarterly Weather Report, 1870, Parts 1, 2. 4to. 1871.
Musée Teyler, Haarlem—Archives Vol. III. Fascicule 2. 4to. 1871.
Peacock, R. A. F.G.S. (the Author)—Changes in the Earth's Physical Geography, &c. (K 98) 8vo. 1871.
Pearson, C. H. Esq. (the Editor)—The Barons' War, including the Battles of Lewes and Evesham. By W. H. Blaauw, M.A. 2nd ed. 1871. 8vo.
Preussische Akademie der Wissenschaften—Monatsberichte, May, June, July, Aug. 1871. 8vo.
Royal College of Surgeons—Descriptive Catalogues of Calculi and of Dermatological Specimens. 2 Parts. 4to. 1870–1.
Royal Society of London—Proceedings, No. 129. 8vo. 1871.
Philosophical Transactions for 1871, Part 1. 4to. 1871.
Catalogue of Scientific Papers, 1800–63. Vol. V. 4to. 1871.
Sargent, F. Esq. (the Author)—Compendium of Biblical Criticism. New edition. 8vo. 1871.
Scott, F. Surtees, Esq. (the Author)—Merlin and Arthur. (K 98) 8vo. 1871.
Smithsonian Institution, U.S.—Annual Report, 1869. 8vo. 1871.
Smithsonian Contributions to Knowledge. Vol. XVII. 4to. 1871.
Société Hollandaise des Sciences—Natuurkundige Verhandelingen, 3me Serie, Tome I. 3. 4to. 1870.
Archives Néerlandaises, Tome V. 4, 5. Tome VI. 1, 2, 3. 8vo. 1870–1.
St. Pétersbourg, Académie Impériale des Sciences—Mémoires, Tome XVI. Nos. 1–8. 4to. 1870.
Bulletins, Tome XV. Nos. 3–5; Tome XVI. No. 1. 4to. 1870–71.
Stanley, The Hon. Wm. Owen, M.P. F.S.A. (the Author)—Memoirs on Remains of Ancient Dwellings in Holyhead Island. With Supplementary Notices by A. Way. 8vo. 1871.
Statistical Society—Journal, Vol. XXXIV. Parts 2, 3. 8vo. 1871.
Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, July, Aug. Sept. Oct. 1871. 8vo.
United Service Institution, Royal—Journal, Nos. 63, 64. 8vo. 1871.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, März bis June, 1871. 4to.
Victoria Institute—Journal of Transactions, Vol. V. Nos. 18, 19. 8vo. 1871.
Wild, M. H. (the Director)—Annales de l'Observatoire Physique Central de Russie, 1866. 4to. 1870.
Repertorium für Meteorologie, Band I. Heft 2. 4to. 1869–70.
Würzburg Physikal-Medecin. Gesellschaft—Verhandlungen, Band II. Heft 3. 8vo. 1871.
Yorkshire Archæological and Topographical Association—Journal, Part 5. 8vo. 1871.
Zoological Society of London—Transactions, Vol. VII. Part 6. 4to. 1871.
Proceedings, 1871, Part 1. 8vo.

GENERAL MONTHLY MEETING,

Monday, Dec. 4, 1871.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Edward Rowe Fisher, Esq.
John Fleming, Esq.
Mrs. G. H. Gibb.
Alfred North, Esq.
John Penn, jun. Esq.
Col. Edward Webb.

were *elected* Members of the Royal Institution.

His Majesty Dom Pedro II. d'Alcantara, Emperor of Brazil, K.G. F.R.S. &c. &c.

was elected an Honorary Member of the Royal Institution.

The following Lecture Arrangements for the ensuing Season were announced:—

Christmas Lectures (adapted to a Juvenile Auditory).

Prof. Tyndall, LL.D. F.R.S.—Six Lectures, On Ice, Water, Vapour, and Air, on December 28, 30, 1871; January 2, 4, 6, 9, 1872.

Before Easter, 1872.

Dr. W. Rutherford, F.R.S.E.—Ten Lectures, On the Nervous and Circulatory Systems, on Tuesdays, January 16 to March 19.

Prof. Odling, F.R.S.—Ten Lectures, On the Chemistry of Alkalies and Alkali Manufacture, on Thursdays, January 18 to March 21.

W. B. Donne, Esq.—Six Lectures, On the Theatre in Shakespeare's Time, on Saturdays, January 20 to Feb. 24.

Moncure D. Conway, Esq.—Four Lectures, On Demonology, on Saturdays, March 2 to 23.

After Easter, 1872.

Dr. Wm. A. Guy, F.R.S.—Three Lectures, On Statistics, Social Science, and Political Economy, on Tuesdays, April 9, 16, and 23.

Edward B. Tylor, Esq. F.R.S.—Six Lectures, On the Development of Belief and Custom amongst the Lower Races of Mankind, on Tuesdays, April 30 to June 4.

Prof. Tyndall, LL.D. F.R.S.—Nine Lectures, On Heat and Light, on Thursdays, April 11 to June 6.

R. A. Proctor, Esq. B.A. F.R.A.S.—Five Lectures, On the Star Depths, on Saturdays, April 13 to May 11.

Prof. Roscoe, F.R.S.—Four Lectures, On the Chemical Action of Light, on Saturdays, May 18 to June 8.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

American Philosophical Society—Proceedings, No. 86. 8vo. 1870.
Asiatic Society of Bengal—Proceedings, Nos. 6, 7, 8. 8vo. 1871.
British Architects, Royal Institute of—Sessional Papers, 1871-2. Nos. 1, 2. 4to.
British Museum Trustees—Guide to the Bronze Room. 8vo. 1871.
Chemical Society—Journal for Nov. 1871. 8vo.
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1872.

WEEKLY EVENING MEETING,

Friday, January 19, 1872.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

William Odling, Esq. M.B. F.R.S.
FULLERIAN PROFESSOR OF CHEMISTRY, R.I.

On the last new Metal, Indium.

The word "element" is used by chemists in a peculiar and very limited sense. In calling certain bodies elements, there is no intention on the part of chemists to assert the undecomposable nature or essence of the bodies so called. There is not even an intention on their part to assert that these bodies may not suffer decomposition in certain of the processes to which they are occasionally subjected; but only to assert that they have not hitherto been proved to suffer decomposition; or, in other words, to assert that their observed behaviour under all the different modes of treatment to which they have been exposed, is consistent with the hypothesis of their not having undergone decomposition.

The entire matter of the earth then, so far as chemists are yet acquainted with it, is composed of some sixty-three different sorts of matter that are spoken of as elementary; not because they are conceived to be in their essence primitive or elementary, but because, neither in the course of nature nor in the processes of art, have they been observed to suffer decomposition. No one of them has ever been observed to suffer the loss of any substance different from the substance of its entirety, so as to leave a residuary substance different from the substance of its entirety. Thus chemists are incapable of taking away from iron, for example, a something that is not iron; or of taking away from it anything whatever, so as to leave a residue that is not iron; whereas they are capable of taking away from iron-pyrites a something which is not iron-pyrites but is sulphur, so as to leave a residue which is not iron-pyrites but is metallic iron.

The notion of all other material bodies being constituted of, and decomposable into a limited number of elementary bodies, which could not themselves be proved to suffer decomposition or mutual transformation under any circumstances whatever, but could, on the contrary, be traced respectively through entire series of combinations, and be extracted at will from each member of the series, is a notion which, undergoing in course of time a gradual development, was first

put forward in a definite form by Lavoisier; until whose time, some residue of the great alchemical doctrine of the essential transmutability of all things—that the substance of all things was the same, while the form above was different—still prevailed. To Lavoisier is due the enunciation of the principle,—departed from, however, in a few instances by himself,—that all bodies which cannot be proved to be compounded, are in practical effect, if not in absolute fact, elementary, and are to be dealt with accordingly.

Of the many definite substances known to chemists before the discovery of hydrogen gas, the following were afterwards recognized by Lavoisier and his colleagues as elementary. First, the seven metals known to the ancients, namely, gold, silver, mercury, copper, iron, tin, and lead, distinguished respectively by the signs of the sun, moon, and planets; and each conceived to have some mystic connection with the particular orb or planet of which it bore the sign, and not unfrequently the name. Then three metals which became known at the latter end of the fifteenth or beginning of the sixteenth century, namely, antimony, discovered by Basil Valentine in 1490; bismuth, mentioned by Agricola, 1530; and zinc, mentioned by Paracelsus, ob. 1541. An elementary character was also assigned to the non-metals carbon and sulphur, which had been known from the earliest times; to phosphorus, discovered by Brandt, of Hamburg, in 1669; and to boracic acid, now known to be a hydrated oxide of boron, first discovered by Homberg in 1702, and still occasionally spoken of as Homberg's sedative salt. The list was further swelled by four metals which, in Lavoisier's time, had been but recently discovered, namely, cobalt and arsenic, identified simultaneously in 1733 by George Brandt, of Stockholm; platinum, discovered in 1741 by Woods, assay-master at Jamaica; and nickel, discovered in 1751 by Cronstedt.

The only other bodies known before 1766, and afterwards included in the class of elements, namely, the alkalies and earths, had during the quarter of a century immediately preceding been made the subjects of especial study. The differentiation of potash from soda, both previously known by the common name of alkali, was indicated by Duhamel in 1736, and more completely established by Marggraf in 1758. The differentiation from one another of lime or calcareous earth, silex or vitrefiable earth, alumina or argillaceous earth, and magnesia or bitter earth, was accomplished by the labour of many chemists, more particularly Marggraf, Bergmann, and Scheele; prior to whose researches silex, alumina, and magnesia, together with their different combinations and commixtures with each other and with lime, were held to be but impure varieties of lime. The nature of the difference between the caustic alkalies and earths and their respective carbonates, was made known by Black in 1756; while the real constitution of the alkalies and earths, as metallic oxides, though suspected by Lavoisier, was not established until the beginning of the present century, by Davy and his cotemporaries and followers.

TABLE I.—ELEMENTS, &c., IN ORDER OF DISCOVERY.

..	GOLD	☉	1774	MANGANESE .	Gahn.
..	SILVER ..	☽	„	BARYTA -IUM.	Scheele.
..	MERCURY ..	☿	1778	MOLYBDENUM	
..	COPPER ..	♀	1781	TUNGSTEN ..	Delhuart.
..	IRON	♂	1782	TELLURIUM ..	Müller.
..	TIN	♃	1789	URANIUM ..	Klaproth.
..	LEAD	♄	„	ZIRCONIA -IUM	
1490	ANTIMONY ..	B. Valentine.	1791	TITANIUM ..	Gregor.
1530	BISMUTH ..	Agricola?	1793	STRONTIA -IUM	Hope.
1541	ZINC	Paracelsus?	1794	YTTRIA -IUM.	Gadolin.
			1797	CHROMIUM ..	Vauquelin.
..	CARBON.		1798	GLUCINA -UM	
..	SULPHUR.		1802	TANTALUM ..	Hatchett.
1669	PHOSPHORUS	Brandt.	1803	CERIUM ..	Klaproth.
1702	BORAX -ON ..	Homberg.	„	PALLADIUM ..	Wollaston.
			„	RHODIUM ..	
1733	ARSENIC ..	G. Brandt.	„	IRIDIUM ..	Descotils & Smithson Tennant.
„	COBALT ..		„	OSMIUM ..	
1741	PLATINUM ..	Woods.	1811	IODINE	Courtois.
1751	NICKEL ..	Cronstedt.	1817	LITHIUM ..	Arfwedson.
			„	SELENIUM ..	Berzelius.
	SODA -IUM ..	Duhamel.	1818	CADMIUM ..	Stromeyer.
	POTASH ..		1826	BROMINE ..	Balard.
1736	LIME	Marggraf.	1828	THORINUM ..	Berzelius.
to	SILEX	Bergmann,	1830	VANADIUM ..	Sefstrom.
1758	ALUMINA ..	and	1839	LANTHANUM .	Mosander.
	MAGNESIA ..	Scheele.	1841	DIDYMIUM ..	
			1843	ERBIUM ..	
			1844	RUTHENIUM .	Claus.
1766	HYDROGEN ..	Cavendish.	1846	NIOBIUM ..	H. Rose.
1771	FLUOR -INE ..	Scheele.	1859	CÆSIUM ..	Bunsen.
1772	NITROGEN ..	Rutherford.	..	RUBIDIUM ..	
1774	CHLORINE ..	Scheele.	1861	THALLIUM ..	Crookes.
„	OXYGEN ..	Priestley.	1863	INDIUM ..	Reich and Richter.

The successive recognition of the elementary gases, quickly following Black's remarkable discovery of carbonic acid gas, began with the identification of hydrogen by Cavendish in 1766. This was succeeded by the discovery of nitrogen by Rutherford in 1772; of chlorine and fluoric acid, the latter now held to be a fluoride of hydrogen, by Scheele in 1774; and of oxygen by Priestley in the same year.

Thus prior to the discovery of the first of the elementary gases, twenty-three kinds of solid matter, and one liquid body mercury, were known, which afterwards became recognized as elements. Between then and the present time, thirty-three kinds of solid matter and one liquid body bromine, have been added to the list—the discovery of the earliest of them occurring almost simultaneously with, or even just preceding, that of the last discovered of the elementary gases.

Among the number of bodies discovered prior to 1808, when Davy effected the decomposition of the alkalies, several, at first

thought to be elementary, are now known to be compounds of oxygen with other bodies still regarded as elements; and conversely, two bodies, namely, chlorine and fluorine, at one time thought to be oxides, have since become regarded as elementary; but in none of these cases did the discovery of what is now considered to be the real constitution of the bodies add or subtract an element to or from the list.

From the period of the modern or Lavoiserian conception of elements and compounds down to the beginning of the nineteenth century, the recognition of new elements occurred with much frequency, at short but varied intervals. After then, the discoveries became somewhat less frequent; but even within the last fifty years, no fewer than twelve new elements have been added to the list, being at the rate of one new element every four years. Throughout, the periods of discovery have been somewhat irregular in their occurrence. Thus in the years 1802 and 1803, six new elements were discovered, namely, tantalum, cerium, palladium, rhodium, iridium, and osmium; within the succeeding fourteen years only one new element, but that a very important one, namely, iodine; and in the fifteenth and sixteenth years, three new elements, namely, lithium, selenium, and cadmium. The longest barren interval, one of thirteen years' duration, took place between the discovery of niobium, by Rose, in 1846, and that of cæsium and rubidium, by Bunsen, in 1859. The last discovered of the elements, namely indium, being fully seven years old, and there being no reason to consider our present list as anything like complete, or to apprehend any cessation of additions thereto, it is now quite time for some other new element to be made known. For we may reasonably anticipate the discovery of new elements to take place at irregular intervals possibly for centuries to come, and our list of the elements to be increased at least as much in the future as in the past.

The fresh discovery, however, of any abundant elementary constituent of the earth's crust would seem scarcely now to be expected, seeing that of the thirty-two elements which have become known since the year 1774,—the year of the discovery of chlorine and oxygen and manganese and baryta,—the great majority belong to the class of chemical curiosities; while even the four or five most abundant of the since discovered elements are found to enjoy but a sparing although wide distribution in nature, as is the case, for example, with bromine and iodine; or else to be concentrated but in a few specially localized minerals, as is the case, for example, with strontium and chromium, and tungsten. Of course it is difficult to appraise the relative abundance in nature of different elements; more especially from the circumstance of those which are put to commercial uses being everywhere sought for, and those not put to commercial uses being habitually neglected,—save indeed by the man of science, to whom the peculiar properties of some of the less familiarly known

elements, as palladium, osmium, erbium, didymium, uranium, and thallium, render them objects of the highest interest.

A very notable point with regard to the last-discovered four elements, namely, rubidium, cæsium, thallium, and indium, is their successive discovery within a few years of each other, by one and the same process, namely, that of spectrum analysis. This process, invented and made available as a means of chemical research by Bunsen and Kirchoff in 1859, consists simply in allowing the light given off by different ignited gases and vapours, limited by means of a fine slit, to pass through a prism or succession of prisms; and in observing the so-produced, brightly-coloured, widely-extended image of the slit. It has been known from the days of Newton, that by the passage of heterogeneous light through a prismatic highly-dispersive medium, its differently refrangible constituents become widely separated from each other, so as to furnish an elongated coloured spectrum. But whereas the spectra of incandescent solid and liquid bodies are continuous, and not distinctive of the particular luminous bodies yielding them, the spectra of incandescent gaseous or vaporized bodies are found to be discontinuous, and to consist of one or more bright lines of different colour, thickness, and position, according to the nature of the particular incandescent gases or vapours from which the light through the slit is proceeding. In this way it is found that the spectra of the different chemical elements, alike when free and in combination, are perfectly definite, and characteristic of the particular elements vaporized and made incandescent.* And in many cases, the spectra or portions of the spectra of particular elements, even when present in the most minute proportion, are so extremely well marked and distinctive, that the presence or absence of these elements is determinable with the greatest ease and certainty, by a mere inspection of the emission spectra yielded by the incandescent gases or vapours under examination. Moreover, gases and vapours are further capable of affecting heterogeneous light which is passed through them; and of thus yielding absorption spectra, in which the characteristic lines of the above-described emission spectra are reversed, so as to appear, unaltered in position, as black lines or intervals in an otherwise continuous band of colour.

Now the salts of the alkali-metals, lithium, sodium, and potassium, and certain of the salts of the alkaline-earth metals, calcium, strontium, and barium, being very readily volatile, upon heating these salts, in the non-luminous flame of a Bunsen gas-burner for example, they undergo vaporization, and their vapours become incandescent and capable of yielding the characteristic emission spectra of the particular metals. In examining in this way the alkali-salt residue of a

* For some qualifications of this statement, *vide* Roscoe's 'Spectrum Analysis.'

mineral water from Durkheim, Bunsen observed in the spectrum before him certain coloured lines not belonging to any one of the then known alkalies, potash, soda, or lithia; and yet necessarily belonging to some substance having the general characters of an alkali, since all other bodies than alkalies had been previously removed from the residue under examination. In full reliance upon the certainty of this conclusion, Bunsen evaporated some forty tons of the water in question; and from the alkali-salt residue, succeeded in extracting and separating salts of two new alkali-metals, each characterized by a well-marked pair of lines in the blue or indigo, and one of them having in addition a pair of well-marked lines of extremely small refrangibility in the red of the spectrum. From its yielding those red lines, the one metal was named rubidium; the other, of which the bright blue lines were especially characteristic, being called cæsium.

The very general distribution in nature of these two elements was speedily established, and salts of each of them were, with much labour, eventually prepared in a state of purity and in reasonable quantity. From certain of their respective salts the metals themselves were obtained by the usual processes, and together with their salts, were submitted to detailed chemical examination. And no sooner was this examination made, than the position of the newly-discovered elements, as members of the alkali-metal family, at once became apparent. Rubidium and cæsium were found in all their properties to present the most striking analogy to potassium, and evidently to stand to this metal in the same relation that strontium and barium respectively stand to calcium; while they differed from sodium, much as strontium and barium respectively differ from magnesium. This relationship in obvious properties was further borne out by the relationship of their atomic weights, thus:—

Mg	24	..	..	Na	23	F	19	..	..	O	16
Ca	40	..	..	K	39	Cl	35·5	..	..	S	32
Sr	87	..	..	Rb	85	Br	80	..	..	Se	79
Ba	137	..	..	Cs	133	I	127	..	..	Te	129

It is observable that the sequence of atomic weight in the thus completed alkali-metal family, is strictly parallel to the previously well-known sequences in the alkali-earth metal family, and in the halogen and oxygen families respectively. Moreover, just as the basylity of the alkaline-earth metals increases in the order of their several atomic weights—calcium being less basylous than strontium, and far less basylous than barium—so also is the basylity of potassium inferior to that of rubidium, and the basylity of rubidium inferior to that of cæsium, which is indeed the most powerfully basylous, or oxidizable, or electro-positive element known.

Since 1860, both rubidium and cæsium have been recognized as minute constituents of a considerable number of minerals and mineral

waters, rubidium having been met with for the most part in a larger proportion by weight than cæsium. Unlike potash, originally known as vegetable alkali, cæsium has not been recognized in the vegetable kingdom; but rubidium has been found as a very common minute constituent of vegetable ashes, as those of beetroot, oak-wood, tobacco, grapes, coffee, tea, &c. On the other hand, cæsium, free from rubidium, has been found in a tolerably well-known, though rare, mineral from the Island of Elba, to the extent of 32 per cent. by weight of the mineral. The history of this mineral is curious: from the circumstance of its always occurring in association with another mineral, a variety of petalite, the two were called Castor and Pollux. Castor was found to be substantially a silicate of alumina and lithia; pollux a silicate of alumina, and, as it was thought, of potash. The constituents of pollux, namely, silica, alumina, and potash, with small proportions of ferric oxide, lime, soda, and water were duly estimated; but the quantities of these constituents, found in 100 parts of the mineral, instead of amounting to 100 parts or thereabouts, amounted only to 88 parts, there being somehow a loss of 12 per cent. in the analysis. After Bunsen's discovery of the new alkali-metals, pollux was analyzed afresh by Pisani, who soon perceived that what had formerly been taken for potash, and estimated as potash, was not potash at all, but cæsia. Then calculating out his own analysis with cæsia instead of potash, substituting the one for the other in the proportion of 133 + 8, or 141 parts of cæsia, for 39 + 8, or 47 parts of potash, he found that the quantities of the different constituents furnished by 100 parts of the mineral yielded by their addition the full sum of 100 parts required.

In submitting to spectroscopic examination a certain residue left by the distillation of some impure selenium, Mr. Crookes, early in 1861, recognized in the spectrum before him a brilliant green line, from which he inferred the presence in the above residue of a new element; and by the end of the same year, he had succeeded in establishing the tolerably wide distribution of this element, to which he gave the name of thallium; in procuring it, though but in small quantity, in a separate state; and in satisfying himself of its metallic character. Soon afterwards, and without knowledge of Mr. Crookes' later results, the metal was obtained by M. Lamy on a comparatively large scale, and was exhibited by him in the form of small ingots at the London Exhibition of 1862. He procured it from the fine dust met with in some oil of vitriol factories, as a deposit in the flues leading from the pyrites burners to the leaden chambers. In these deposits, the minute proportion of thallium contained originally in the pyrites becomes concentrated, so as to form in some instances as much as 8 per cent. by weight of the dust. Independently, moreover, of its occurrence in iron pyrites, thallium, though never forming more than a minute constituent of the different minerals and mineral waters in which it occurs, is now known to be capable of extrac-

tion from a great number and variety of sources. But from no other source is it so advantageously procurable as from the above-mentioned flue deposit; and so early as the autumn of 1863, at the meeting of the British Association in Newcastle, the then mayor, Mr. J. Lowthian Bell, exhibited several pounds, and Mr. Crookes no less than a quarter of a hundredweight of thallium obtained from this comparatively prolific source. In one respect, the discovery of thallium presented even a greater degree of interest than attached to the discovery of cæsium and rubidium. For whereas these two elements were at once recognized as analogues of the well-known metal potassium, thallium can hardly be said, even at the present time, to be definitely and generally recognized by chemists as the analogue of any particular metal, or as a member of any particular family of elements. With each of such differently characterized elements as potassium, lead, aluminum, silver, and gold, it is associated by certain marked points of resemblance; while from each of them it is distinguished by equally well-marked points of difference. Hence the necessity for subjecting thallium and its salts to a thorough chemical examination, so as to accumulate a well-ascertained store of facts with regard to it. And thanks to the careful labours of many chemists, more particularly of Mr. Crookes in London, and of Messrs. Lamy and Willm in Paris, our knowledge of the properties of thallium and of its salts may compare not unfavourably with our similar knowledge in relation to even the longest known of the metallic elements. Still it was not until our knowledge of indium had culminated in the determination of its specific heat only last year, that the position of thallium as an analogue of indium, and a member of the aluminum family of elements became unmistakably evident.

Indium was first recognized in 1863, by Drs. Reich and Richter, in the zinc blende of Freiberg in Saxony, and by reason of the very characteristic spectrum afforded,—consisting of two bright blue or indigo bands; the brightest of them somewhat more refrangible than the blue line of strontium, and the other of them somewhat less refrangible than the indigo line of potassium. Since its first discovery, indium has been recognized in one or two varieties of wolfram, and as a not unfrequent constituent of zinc ores, and of the metal obtained therefrom, but always in a very minute proportion. Indeed, indium would appear to be an exceedingly rare element, far more rare than its immediate predecessors in period of discovery. Its chief source is metallic zinc,—that of Freiberg, smelted from the ore in which indium was first discovered, containing very nearly one-half part of indium per 1000 parts of zinc. A considerable quantity of indium extracted from this zinc, was shown in the Paris Exhibition of 1867; and an ingot from the Freiberg Museum, weighing 200 grammes, or over 7 ounces, has within the last few days been kindly forwarded by Dr. Richter himself, for inspection on the present occasion. To Dr. Schnchardt, of Goerlitz, also, the members of the Institution are

indebted for his loan of nearly 60 grammes of metallic indium; and of fine specimens of other rare chemical products, prepared with his well-known skill, in a state of great purity and beauty.

When zinc containing indium is dissolved not quite completely in dilute sulphuric or muriatic acid, the whole of the indium originally present in the zinc is left in the black spongy or flocculent residue of undissolved metal, with which everyone who has prepared hydrogen gas by means of zinc and acid is so well acquainted. Besides some zinc, this black residue is found to contain lead, cadmium, iron, and arsenic, less frequently copper and thallium, and in some cases, as that of the Freiberg zinc, a small proportion of indium. From the solution of this residue in nitric acid, the indium is separated by ordinary analytical processes, based chiefly on the precipitability of its sulphide by sulphuretted hydrogen from solutions acidulated only with acetic acid; and on the precipitability of its hydrate both by ammonia and carbonate of barium. From its soluble salts, metallic indium is readily thrown down in the spongy state by means of zinc. The washed sponge of metal is then pressed together between filtering paper, by aid of a screw press, and finally melted under a flux of cyanide of potassium.

Thus obtained, indium is a metal of an almost silver-white colour, apt to become faintly bismuth-tinted. It tarnishes slowly on exposure to air, and thereby acquires very much the appearance of ordinary lead. Like lead, it is compact and seemingly devoid of crystalline structure. Moreover, like lead and thallium, it is exceedingly soft, and readily capable of furnishing wire, by the process of "squirting" or forcing. The specific gravity of indium, or 7·4, is very close to that of tin, or 7·2; and much above that of aluminum, 2·6, and below that of lead, 11·4, and that of thallium, 11·9. In the lowness of its melting point, namely 176° C., indium occupies an extreme position among the metals permanent in air; the next most fusible of these metals, namely tin and cadmium, melting at 228°; bismuth at 264°; thallium at 294°; and lead at 235°. Though so readily fusible, indium is not an especially volatile metal. It is appreciably less volatile than the zinc in which it occurs, and far less volatile than cadmium. Heated as far as practicable in a glass tube, it is incapable of being raised to a temperature sufficiently high to allow of its being vaporized, even in a current of hydrogen.

Indium resists oxidation up to a temperature somewhat beyond its melting point, but at much higher temperature it oxidizes freely; and at a red heat, it takes fire in the air, burning with a characteristic blue flame and abundant brownish smoke. It is readily attacked by nitric acid, and by strong sulphuric and muriatic acids. In diluted sulphuric and muriatic acids, however, it dissolves but slowly, with evolution of hydrogen. Oxide of indium is a pale yellow powder, becoming darker when heated, and dissolving in acids with evolution of heat. The hydrated oxide is thrown down from indium solutions by ammonia, as a white, gelatinous, alumina-like precipitate, drying up into a horny mass. The

sulphide is thrown down by sulphuretted hydrogen as an orange-yellow precipitate, insoluble in acetic but soluble in mineral acids. The hydrate and sulphide of indium, in their relations to fixed alkali solutions more particularly, seem to manifest a feebly-marked acidulous character. Chloride of indium, obtained by combustion of the metal in chlorine gas, occurs as a white micaceous sublimate; and is volatile at a red heat, without previous fusion. The chloride itself undergoes decomposition when heated in free air, and the solution of the chloride upon brisk evaporation, with formation in both cases of an oxichloride.

But the chief point of chemical interest with regard to any newly-discovered element, and consequently with regard to indium, is the establishment of its atomic weight; which, in the case of a metallic element, is based primarily upon the determination of the ratio in which it combines with oxygen and chlorine. Now the quantity of indium which unites with 8 parts by weight of oxygen and with 35·5 parts by weight of chlorine, has been found by Winkler to be 37·9, and by Bunsen to be 37·8 parts. But this determination of combining ratio falls far short of the definite establishment of the atomic weight of the metal. For example, the quantities of silver, mercury, bismuth, tin and tantalum, which exist in the best-known chlorides of these metals combined with 35·5 parts of chlorine, are 108, 100, 70, 29, and 37 parts respectively. Nevertheless, the atomic weights of these metals are taken to be not 108, 100, 70, 29, and 37, but 108, 200, 210, 118, and 184 respectively, the chlorides of the several metals being expressed by the formulæ $Ag Cl_1$, $Hg Cl_2$, $Bi Cl_3$, $Sn Cl_4$, and $Ta Cl_5$, respectively. Accordingly, in order to deduce the atomic weight of indium from the ascertained composition of its chloride, we require first to know whether its chloride is a mono-, di-, tri-, tetra-, or penta-chloride. Now, in the case of a metal forming only one definite chloride, the constitution of the chloride as a mono- or poly-chloride, may frequently be determinated by a consideration of the analogies presented by the metal and its compounds to some other metal and its compounds, of which the atomic weight and molecular formulæ respectively are well established. But it is obvious that analogy can afford but little help in the case of a newly-discovered element, of which the analogies have still to be determined.

Failing analogy, a more sure guide to the establishment of the molecular formula of a metallic chloride is afforded in some instances by a determination of its vapour density,—tantamount to a determination of the quantity of chlorine by weight, contained in a given volume of the gas or vapour of the chloride experimented on. Thus, having estimated the quantity of chlorine contained in a given volume of heated muriatic acid gas, the quantities of chlorine contained in the same volume of the vaporized chlorides of mercury, bismuth, tin, and tantalum, under the same circumstances of pressure and temperature, are found to be 2, 3, 4, and 5 times as great, whence the formulæ $H Cl_1$, $Hg Cl_2$, $Bi Cl_3$, $Sn Cl_4$, and $Ta Cl_5$, respectively. Now indium chloride being volatile at a red heat, there is no reason, save that re-

sulting from the rarity and value of the body, why the density of its vapour should not be ascertained. As a matter of fact, however, no estimation of the vapour density of indium chloride has yet been made, and any evidence that might be deducible from it, is consequently not forthcoming.

Lastly, a most important guide to the establishment of the atomic weight of a metal is the determination of its specific heat. In cooling through the same fall of temperature, different bodies, as is well known, give out exceedingly different quantities of heat. In the case of a pound of bismuth and a pound of brass for instance, both raised to the temperature of boiling water, and then immersed in an excess of ice, the quantity of ice melted by the pound of brass in cooling down to the freezing point, will be found to be more than three times as great as the quantity of ice melted by the pound of bismuth. Now the determination of the specific heats of most of the metals, compared with the specific heat of an equal weight of water as unity, has been made with extreme care and exactitude by Regnault; and on looking at the following list of specific heats, mostly of his determination, it is evident, almost at a glance, that the specific heats of the metallic elements are inversely as their respective atomic weights. Thus, taking the first and last elements on the list for example, it is observable that the specific heat of lithium, or 0·94, is weight for weight thirty times greater than the specific heat of bismuth, 0·03; but then the atomic weight of bismuth is thirty times greater than that of lithium. And throughout, the product of the specific heat into the atomic weight of one metal, divided by the product of the specific heat into the atomic weight of another metal, is approximatively equal to 1, as shown in the fourth column of the following Table, in which the product of the specific heat into the atomic weight of silver is taken as the standard dividend. Now, only last year, concordant estimations of the specific heat of indium were made by Bunsen and a Russian chemist, Mendelejeff; the mean of Bunsen's two estimations being 0·0569, which it will be observed is very close to Regnault's estimations of the specific heats of silver, cadmium, and tin. Accordingly, the atomic weight of indium must approximate to the atomic weights of silver, cadmium, and tin; or, in other words, it cannot be $37 \cdot 8 \times 1$, or $37 \cdot 8 \times 2$, but must be $37 \cdot 8 \times 3 = 113 \cdot 5$; and the quantity of chlorine combined with this weight of indium being three times 35·5 parts, indium chloride will necessarily appear as a trichloride, and be expressed by the formula $InCl_3$. The determination of specific heats being a matter of direct experiment, with scarcely any ratiocination whatever, it seems impossible for anyone to observe the relationship subsisting between the accepted atomic weights of the metals, deduced from experiment by a highly complex train of reasoning, and their directly ascertained specific heats, without recognizing that in the case of the metals, at any rate, the atomic weights of the chemist are something more than vain imaginings, but that they are beyond question the terse expression of a fundamental truth in nature.

TABLE II.—ATOMIC HEATS OF METALS.

Chlorides.	Atomic Weights.	Metals.	Specific Heats.	Atomic Heats.
$LiCl$	7	Lithium	·9408	1·07
$NaCl$	23	Sodium	·2934	1·09
$MgCl_2$	24	Magnesium	·2499	0·97
$AlCl_3$	27·5	Aluminum	·2143	0·95
KCl	39	Potassium	·1695	1·07
$CaCl_2$	40	Calcium	·1686	1·09
$MnCl_2$	55	Manganese	·1217	1·08
$FeCl_2$, $FeCl_3$ ..	56	Iron	·1138	1·03
$NiCl_2$	59	Nickel	·1075	1·03
$CoCl_2$	59	Cobalt	·1067	1·02
$CuCl$, $CuCl_2$ ..	63·5	Copper	·0955	0·98
$ZnCl_2$	65	Zinc	·0955	1·01
$AsCl_3$	75	Arsenic	·0814	0·99
$MoCl_4$	96	Molybdenum ..	·0722	1·12
$RuCl_3$, $RuCl_4$ ..	104	Ruthenium	·0611	1·03
$RoCl_3$	104	Rhodium	·0580	0·98
$PdCl_2$	106	Palladium	·0593	1·02
$AgCl$	108	Silver	·0570	1·00
$CdCl_2$	112	Cadmium	·0567	1·03
$InCl_3$	113·5	Indium	·0569	1·05
$SnCl_2$, $SnCl_4$ ..	118	Tin	·0562	1·07
$SbCl_3$, $SbCl_5$ ..	122	Antimony	·0508	1·00
$TeCl_4$	129	Tellurium	·0474	1·03
WCl_4, WCl_6 ..	184	Tungsten	·0334	1·00
$AuCl$, $AuCl_3$ ..	196·5	Gold	·0325	1·03
$IrCl_3$, $IrCl_4$	197	Iridium	·0326	1·04
$PtCl_2$, $PtCl_4$ ..	197	Platinum	·0324	1·04
$OsCl_2$, $OsCl_4$ ..	199	Osmium	·0311	1·00
$HgCl$, $HgCl_2$ ..	200	Mercury	·0319	1·03
$TlCl$, $TlCl_3$	203	Thallium	·0325	1·07
$PbCl_2$	207	Lead	·0314	1·05
$BiCl_3$	210	Bismuth	·0308	1·05

The most important chemical characters of indium being thus established, there remains for consideration only the question of its affinities to certain of the previously-known elements. And seeing that the atomic weights of the elements range from 1, the atomic weight of hydrogen, up to 240, the atomic weight of uranium, there opens out the further question, whether the more obvious chemical properties of the different elements are seriated in any way with their atomic weights; or to put this last question in another form, whether the varied chemical properties of the elements are distributed among them haphazard, or according to some definite system of which the relationship subsisting between their several atomic weights may possibly serve as a key. Now the atomic weights, as distinguished from the combining proportions, of yttrium, erbium, cerium, lanthanum, and didymium, must be regarded for the present as quite unknown. Out of the fifty-eight elements, however, of which the atomic weights have been more or less well determined, forty-six have their several atomic weights ranging from 1 to 137, in an almost unbroken succession. Ten of the other twelve have atomic weights ranging from 184, that of tantalum, to 210, that of bismuth; while the remaining two,

TABLE III.—ELEMENTS, IN ORDER OF ATOMIC WEIGHT.*

1.	2.	3.	4.	5.	6.	7.	8.	Type.
I. H 1	Li 7	Na 23	K 39	..	Rb 85	Ag 108	Cs 133	RCl
II.	G 9	Mg 24	Ca 40	Zn 65	Sr 87·5	Cd 112	Ba 137	RCl_2
III.	B 11	Al 27·5	..	..	X^a	In 113	X^b	RCl_3
IV.	C 12	Si 28	Ti 50	..	Zr 89	Sn 118	X^c	RCl_4
V.	N 14	P 31	V 51	As 75	Nb 94	Sb 122	..	RCl_5
VI.	O 16	S 32	Cr 52·5	Se 79	Mo 96	Te 129	..	RCl_6
VII.	F 19	Cl 35·5	Mn 55	Br 80	..	I 127	..	RCl_7
VIII.	..	..	Fe 56 Co 59 Ni 59	..	Ru 104 Ro 104 Pd 106	..	..	RCl_8
	Na 23	..	Cu 63·5	..	*Ag* 108			

namely, thorium and uranium, have the closely-approximating atomic weights 238 and 240 respectively. In the above Table, the symbols of the forty-six elements having atomic weights ranging from 1 to 137,

* This Table is based on one published by the author in 1864–5. Similar tables have been constructed by Newlands, Meyer, Mendelejeff, and others. The positions marked X^a, X^b, and X^c, are assigned by Mendelejeff to yttrium, didymium, and cerium, respectively. The recognition of the atomic weight of uranium as 240, is also due to Mendelejeff.

are set down in the order of the atomic weights of the elements symbolized,—save only in the case of tellurium, of which the symbol is placed immediately above, instead of below that of iodine, and of which the atomic weight may not improbably have been somewhat over-estimated. And violating the order of numerical seriation in this small particular only, it is remarkable with what facility the symbols of the forty-six elements may be arranged in parallel lines and columns, corresponding to a natural classification of the elements themselves into analogous groups and series. Indeed, a study of the entire number of elements at present known, would seem to indicate that they are one and all associated with each other by a certain community of relationship; of which the well-known gradation and parallelism in properties and atomic weights, of the members of the alkali and earth-alkali, and of the halogen and oxygen families of elements afford only the most prominent examples.

Taking the second line of the Table as an illustration, it is observable that the seven metals symbolized thereon are distinguished from all the others by their common property of forming one chloride only, and that a di-chloride; further, that the metals figuring in the uneven-numbered columns of this line, namely, magnesium 24, zinc 65, and cadmium 112, are permanent in the air, are volatilizable in the direct, and basylous in the inverse order of their atomic weights, and are otherwise specially associated with one another; while the similarly associated metals of the alternate or even-numbered columns, namely, calcium 40, strontium 87·5, and barium 137, are quickly oxidizable in the air, are practically non-volatile, and are basylous in the direct instead of in the inverse order of their atomic weights; and similarly, on the other lines of the Table, the elements symbolized are divisible into sub-groups, according to their odd and even positions respectively.

Such being the relationship of the elements placed on the same line, the relationship of those in the same column is of a different kind. Taking the third and seventh columns by way of illustration, it is observable that the consecutive elements in each column have closely consecutive atomic numbers; that the element on the first line forms a mono-chloride; that on the second line, a di-chloride; that on the third line, a tri-chloride; and that on the fourth line, a tetra-chloride; while those on the fifth, sixth, and seventh lines form oxides or oxichlorides, corresponding to a penta-, hexa-, and hepta-chloride respectively.

By reason of its atomic weight, 113·5, indium is observed to figure on the third line and seventh column of the above Table; but its position among the elements is better recognizable by a glance at the Table below, containing a portion only of the preceding one, supplemented by an additional column of elements of higher atomic weight than any of those included previously.

In respect of its atomic weight, then, triad indium occupies a position exactly intermediate between the positions of diad cadmium and tetrad tin, to both of which metals it presents a most marked re-

semblance in properties. They all three have the same extreme degree of fusibility, and much the same oxidizability and reducibility. Their sulphides are alike characterized by a yellow colour, that of cadmium, Cd S, being neutral; that of tin, $Sn S_2$, being acidulous; and that of indium, $In_2 S_3$, being strictly intermediate.

	8.	7.	10.	Type.
I.	Na 23	Ag 108	..	R Cl
II.	Mg 24	Cd 112	Hg 200	$R Cl_2$
III.	Al 27·5	In 113·5	Tl 203	$R Cl_3$
IV.	Si 28	Sn 118	Pb 207	$R Cl_4$
V.	P 31	Sb 122	Bi 210	$R Cl_5$
VI.	S 32	Te 129	..	$R Cl_6$

Viewed in another aspect, triad indium occupies a position intermediate between the positions of its remote triad congeners, aluminium and thallium. The mean atomic weight of the three metals being 114·3, the atomic weight of indium is 113·5. The mean specific gravity of the three metals being 7·3, the specific gravity of indium is 7·4. And in respect of purely chemical habitudes, hydrated alumina and hydrated india might easily be mistaken for one another. It is interesting, moreover, to remark that the last-discovered two metals indium and thallium—discovered, it will be remembered, by the same process, that of spectrum analysis—should bear to one another much the same sort of relation that is borne to one another by the jovian and saturnine metals of the alchemical or even pre-alchemical era. Just, for example, as the unstable and least-known chloride of lead, $Pb Cl_4$, corresponds to the stable chloride of tin, $Sn Cl_4$, so does the unstable and least-known chloride of thallium, $Tl Cl_3$, correspond to the stable, and as yet only known, chloride of indium, $In Cl_3$, as suggested, indeed, by the lecturer some six or seven years ago.

The study of such relationships necessarily suggests many inquiries. Arranging the entire fifty-eight elements of which the atomic weights are known, in a table similar to the preceding one for the forty-six elements having atomic weights not exceeding 137, some twenty or five-and-twenty new elements would be required to fill up the gaps in the different series; but why should not new elements be discovered having atomic weights as much above that of uranium, 240, as its atomic weight is above that of barium, 137?

Again, does it seem probable that bodies capable of being arranged in such a well-marked numerical series, are really elementary and mutually independent; or is it more likely that the gradation of properties and atomic numbers manifested by these bodies, depends on their possession of different increments of common material?

May it not be that the numerical ratio between the atomic numbers of proximate elements, $\frac{x}{y}$ = approximatively $\frac{y}{z}$, is really absolute; and that it will hereafter be proved to be so by a better determination of atomic weights. Seeing that a short time back, cæsium with the atomic weight 133, and rubidium with the atomic weight 85, both occurred as unrecognized impurities in potassium with its atomic weight 39, who shall answer for the absolute accuracy of even the best established of our present atomic weights?

Again, the mean difference in atomic weight between consecutive analogous elements, is, in the case of the nine following pairs of elements, lithium and sodium, glucinum and magnesium, boron and aluminum, carbon and silicon, nitrogen and phosphorus, oxygen and sulphur, fluorine and chlorine, sodium and potassium, magnesium and calcium, 16·1; the lowest difference being 15, and the highest 17. The mean difference in the case of the four following similar pairs of proximate elements, phosphorus and vanadium, sulphur and chromium, chlorine and manganese, arsenic and niobium, is 19·25; the lowest difference being 19, and the highest 20·5. Lastly, the mean difference in the case of the seven following similar pairs of proximate elements, calcium and zinc, vanadium and arsenic, manganese and bromine, rubidium and silver, strontium and cadmium, silver and cæsium, tantalum and bismuth, is 24·6; the lowest and highest differences, even in the case of these elements of such high atomic weight, being 23 and 26 respectively. Are these differences in atomic weight only approximatively, or are they indeed absolutely, 16, 20, and 24 respectively; and if so, why should the numerical difference between proximate associated elements be 16 in one set of cases, 20 in another set, and 24 in a third?

[W. O.]

WEEKLY EVENING MEETING,

Friday, January 26, 1872.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Henry Edward Archbishop of Westminster,

On the Dæmon of Socrates.

At the outset I must plead guilty to a misnomer, for which, however, I am not responsible. It has become a tradition to speak of the Dæmon of Socrates; but I hope to show that the term is without warrant and incorrect.

The Dæmon of Socrates has been treated so often, and by so many authors, historians, philosophers, and critics, both in classical and Christian times, that I, at least, cannot hope to say anything new upon it. I may, however, review the judgments of others, and then offer what seems to me to be the true interpretation of this singular fact in the history of philosophy.

It will, I think, be found to be no mere intellectual eccentricity, no mere superstition, still less an unmeaning record of Greek history, but a fact in the psychology of the greatest philosophical mind of the ancient world, full of significance for us, and throwing much light upon the analysis of our moral nature.

The life of Socrates extended over a tract of seventy years, that is from 469 to 400 B.C., and embraced the most critical and splendid period of Athenian history. . . . . In the midst of all the splendours of imperial greatness, intellectual culture, excessive refinement, luxurious self-indulgence, public and private immorality, Socrates arose as a cross-questioner of men, a seeker after moral truth, an example and a teacher of temperance and justice. There is something majestic and melancholy in his account of himself, and of his mission, as he declared it in his Apology before his judges. He was accused by Meletus and Anytus of corrupting the youth of Athens by philosophical paradoxes, and of introducing new gods, or of denying all gods. In answer he spoke as follows:—

"If you should say to me, 'O Socrates, we will not believe Anytus. We will let you off; but on this condition, that you no longer go on with this questioning and philosophizing; and if you should be caught again doing this, you shall die.'

"If, as I said, you should acquit me on these conditions, I should say to you,

O men of Athens, I reverence you and I love you, but I shall obey God rather than you. As long as I breathe, and am able, I shall not cease to philosophize, and to exhort you, and to demonstrate (the truth) to whomsoever among you I may light upon, saying, in my accustomed words, 'How is it, O best of men, that you, being an Athenian, and of a city the greatest and noblest for wisdom and power, are not ashamed to be careful of money, studying how you can make the most of it; and of glory also and of honour; but of prudence, and truth, and of the soul, how you may make the best of that, have neither care nor thought?' And this I will do, to young and old, whomsoever I may meet; both to alien and citizen, and, above all, to the men of this city, inasmuch as you are nearer to me in kindred. For this is the command of God, as you well know; and I think that no greater good ever yet came to the State, than this service which I render to God. For I go about doing nothing else than to persuade you, both young and old, to be careful in the first place neither of the body, nor of money, nor of anything so earnestly as of the soul, how you may make it as perfect as possible. I tell you that virtue does not spring from money, but that from virtue money springs, and all other goods of man, both to the individual and to the commonwealth. If, then, to teach these things be to destroy our young men, that would be mischievous in me indeed. But if anyone should say I teach anything other than these truths, he speaks falsely. Moreover, I say, O Athenians, whether you believe Anytus or not, and whether you let me go or not, I shall never do anything else, even though I were to die many times.*

"Do not clamour, O Athenians, but abide by the request I made to you, that is, not to clamour at what I am saying, but to hear me. For you will be benefited, I believe, by hearing me. I am about to say to you some things at which, perhaps, you will cry out; but I pray you not to do so. For you know well, if you should kill me, being such a one as I say I am, you will not hurt me so much as you will hurt yourselves. Neither Meletus nor Anytus can any way hurt me. This cannot be. For I do not think that it is ever permitted that a better man should be hurt by a worse. Perhaps, indeed, he may kill him or drive him into exile, or disfranchise him; and these things perhaps he and others may think to be great evils. But I do not think so: much rather the doing that which he (Meletus) is now about—the laying hands on a man to kill him unjustly, is a great evil. But, O Athenians, I am far from making now a defence for myself, as some may think; [I am making it] in your behalf; lest by condemning me you should in anything offend in the matter of this gift which God has given you. For if you should kill me, you will not easily find another man like me, who, to speak in a comic way, is so precisely adapted by God to the State; which is like a horse, large and well-bred, but from its very size sluggish, and needing to be roused by some gad-fly. For so it seems to me, that the God has applied me, such as I am, to the State, that I may never cease to rouse you, and persuade and shame everyone, fastening upon you everywhere all day long. Such another will not easily come to you, O men of Athens; and if you will listen to me, you will spare me. But perhaps, as those who awake in anger when they are stung, you will, at the instigation of Anytus, kill me at once with a slap; then you will end the rest of your life in sleep, unless God shall send some other gad-fly to be mindful of you. But that I am such a one, given by God to the State, you may know from this fact. It is not like the way of men that I, now for so many years, should have disregarded all my own concerns and should have endured the neglect of my own domestic affairs, and should have been ever busied about your interests; going about to each of you privately, as a father or an elder brother, persuading you to be careful of virtue. If indeed I had derived any enjoyment from these things, and for these exhortations had received any reward, there would have been some reason in it. But now you yourselves see that the accusers, charging me as they do, without shame, of other things, of this at least have not been able to bring a witness against me; as if I had ever either exacted or asked

* Apologia Socratis, s. 17. Platonis Opp., vol. i. 114. Ed. Stallbaum, Gothæ, 1858.

any reward. I think, moreover, that I adduce a sufficient witness that I speak the truth—I mean my poverty.*

"It may perhaps appear strange that I should go to and fro, giving advice, and busying myself about these things in private, but that in public I should not venture to go up (*i. e.* to the Pnyx) to give counsel to the State before your assembly. But the cause of this is what you have heard me say often and in many places; that a voice is present with me—a certain agency of God, somewhat divine (δαιμόνιον)—which indeed Meletus has caricatured and put into the indictment. Now this began with me from my childhood; a certain voice, which always, when it comes, turns me aside from that which I am about to do, but never impels me to do anything. It is this which opposed my mixing in politics, and I think very wisely. For you well know, O Athenians, that if I had been hitherto mixed in political matters, I should have perished long ago; and should have done no good, either to you or to myself. Do not be angry with me for speaking the truth; for there is no man who will save his life if he shall courageously oppose either you or any other populace, by striving to hinder the multitude of unjust and lawless things which are done in the State. It is necessary, therefore, that anyone who really combats for the sake of justice, if he would survive even for a little while, should live a private and not a public life."†

When Socrates had ended his defence, the votes were taken: first, he was condemned as guilty of the charges laid against him; and, secondly, he was sentenced to die. He then once more addressed the court.

"I would wish to speak kindly with those who have voted for me, in respect to what has now happened; while the archons are occupied, and before I go to the place where I must die. Bear with me, therefore, O Athenians, for such time as we have. While it is so permitted, nothing forbids our conversing together. I wish to show to you, as my friends, what is the meaning of that which has now befallen me. O my judges—for in calling you judges I should call you rightly—something marvellous has happened to me. Hitherto, the Oracle of the δαιμόνιον, which is familiarly about me, with great frequency has opposed itself, even in very little things, if I were about to act in any way not rightly. But now there has befallen me, as you yourselves see, that which men may think, and most men do account, to be the greatest of evils. And yet this morning, neither when I came from home did the sign from the God oppose itself, nor when I came up hither to the court of judgment, nor anywhere during the defence I was about to make; although in other speeches it has often restrained me in the very midst of speaking. But now in this affair it has not anywhere opposed me, either in any deed or word. What, then, do I suppose to be the cause? I will tell you. That which has happened to me seems to be a good thing; and if we think death to be an evil, we are in error. Of this I have a sure evidence; for it cannot be that the accustomed sign would not have opposed itself to me, if I were not about to do something which is good.‡

"Wherefore, O my judges, you ought to be of good hope about death; and to know this to be true, that no evil can happen to a good man, whether in life or in death; nor are his affairs neglected by the gods. Nor are my affairs at this time the result of chance. But this is clear to me, that it were better for me now to die, and to be set free from troubles. Wherefore the sign has in nothing opposed me. I am, therefore, in no way angry with those who have condemned me, nor with those who have accused me; though they have condemned and accused me with no good will, but rather with the thought to hurt me. This, indeed, in them, is worthy of blame."§

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* Apologia Socratis, s. 18, p. 118. † Ibid. s. 19, p. 123.
‡ Ibid. s. 31, p. 164. § Ibid. s. 33, p. 172.

Such was his general defence against his accusers. He stood up as a man conscious of something in him higher than himself; of a calling and a mission to his countrymen. He had laboured to recall them from luxury, self-indulgence, ambition, civil strife, political profligacy, and private corruption. He told them roundly that no man could serve them who mixed in their politics—that no man could rebuke their corruptions and live. Therefore it was that Meletus and Anytus accused him; and their accusation was the expression of a wide-spread enmity in Athens.

The charges laid against Socrates were chiefly two: the one, that of corrupting the youth of the day by his philosophy; the other, that of impiety, and of introducing new gods, ἕτερα καινὰ δαιμόνια, or of denying the existence of gods. It is with the latter we have chiefly to do, because it connects itself with the belief of Socrates in respect to the Dæmonion, or voice, or sign, which from his childhood had been with him as a monitor and guide.

In answer to the charge of atheism, Socrates asked his accuser:—

"Is there any man who believes that there are human affairs, but does not believe in the existence of men; or that there are certain rules for managing horses, and yet believes that there is no such thing as a horse? There is no such man. But pray answer me this point: is there any man who believes divine things and yet denies the being of a God?" Meletus answered, "No, certainly." Then Socrates replied: "You acknowledge, then, that I believe and teach the existence of Deities. So that, whether they be new or old, you still own that I believe in divinities or divine agencies. Now if I believe that there are divinities or divine agencies, I must necessarily suppose that there are Gods." *

In these passages of Plato we have the fullest and most explicit declaration of Socrates respecting the Dæmon by which he was admonished. He tells us that it was a "familiar sign; an oracle; a divine voice:" that this sign had been with him from his infancy: that its office was to take him off from certain lines of action; that it did not impel him to any.

With such declarations before him, it is not wonderful that Plutarch should have supposed this Dæmon to be a personal being, and that he should have written a book, 'De Genio Socratis,' on the 'Familiar Spirit of Socrates;' and that Apuleius should have written 'De Deo Socratis,' of 'The God of Socrates:' and that the Neoplatonists and certain of the Christian Fathers should have understood this Dæmonion to be a personal being or genius: whether good or bad, they did not determine.

Plutarch has not promoted either the perspicuity or the gravity of the subject by telling us that "a voice in the cave of Trophonius expounded to Timarchus the philosophy of dæmons in the following words:—

"Every soul partakes of reason. It cannot be without reason and intelligence. But so much of each soul as is mixed with flesh and passions is changed, and through pain or pleasure becomes irrational. Every soul does not mix itself in the same manner. For some plunge themselves altogether into the body, and so

* Apologia Socratis, s. 15, p. 105.

in this life their whole frame is corrupted by appetite and passion; others are mixed only in part. but the purer part still remains out of the body. It is not drawn down into it; but it floats above, and touches the upper part of a man's head. It is like a cord to hold up and direct the part of the soul which is sinking, as long as it proves obedient,' and is not overcome by the passions of the flesh. The part that is plunged into the body is called the soul; but the uncorrupted part is called the mind, and people think that it is within them: as likewise they imagine the image reflected from a glass is in the glass. But the more intelligent, who know it to be external, call it a dæmon." *

"Such was the soul of Hermodorus the Clazomenian, of which it is reported that for nights and days it would leave his body, travel over many countries, and return, after it had seen things and talked with persons at a great distance; till at last, by the treachery of his wife, his body was delivered to his enemies; and they burnt it in his own house while the soul was abroad." Plutarch considerately adds: "It is certain that this is not true;" but he goes on to say: "The soul never went out of the body, but it loosened the tie that held the dæmon, and gave it range and freedom." †

Plutarch then relates the following anecdote:—

"More and greater things you may learn from Simias, and other companions of Socrates; but once, when I was present, as I went to Euthyphron the soothsayer, it happened, Simias, as you remember, that Socrates was going up to Symbolum, and the house of Andocides, all the way asking questions, and playfully attacking Euthyphron. When, suddenly standing still, and making us to do the same, he pondered with himself for some time. Then, turning about, he walked through Trunkmakers' Street, calling back his friends that walked before him, affirming that it was because of his dæmon. Many turned back, amongst whom I, holding Euthyphron, was one; but some of the youths, keeping on the straight road, in order, as it were, to disprove the dæmon of Socrates, took along with them Charillus the piper, who came with me to Athens to see Cebes. Now, as they were walking through Sculptors' Street, near the court houses, a herd of pigs, covered with mud, met them; and, being too many for the street, and running against one another, they upset some that could not get out of the way, and dirtied others; and Charillus came home with his legs and clothes very muddy; so that often, in merriment, they would remember Socrates' dæmon, wondering at its constant care of the man, and that Heaven kept such a particular watch over him." ‡

"I myself, Galaxidorus, have heard a Megarian, who had it from Terpsion, say that Socrates' dæmon was nothing else but the sneezing either of himself or of others; for if another sneezed, either before, behind him, or on his right hand, then he went on to do what he was about; but if on the left hand, he refrained from acting. One sort of his own sneezing confirmed him, whilst deliberating and not fully resolved; another stopped him when already about to act. But indeed it seems strange that if he used sneezing as his sign, he should not have told this to his friends, but should have said that it was a dæmon that hindered or enjoined him." §

The following passage is more to our purpose. Plutarch says: —

"The resolute impulses of Socrates seem to be both vigorous and firm, as springing from right principles and strong judgment. Therefore he, of his own will, lived in poverty all his life, though he had friends who would have been glad and willing to give to him; he would not give up philosophy, notwithstanding all the discouragements he met with; and at last, when his friends endeavoured and skilfully contrived his escape, he would not yield to their entreaties, nor with-

* Plutarch, De Genio Socratis, sect. xxii. Moralia. Tom. i. 713. Ed. Dœhner, Paris, 1841.

† Ibid. ‡ Ibid. sect. x. § Ibid. sect. xi.

draw from death, but maintained an inflexible mind in the last extremity. And surely these are not the actions of a man whose designs, when once fixed, could be altered by omens or sneezings; but of one who, by some higher guidance and principle, is directed to do right."*

Plutarch then says that Socrates foretold the overthrow of the Athenians in Sicily; and that in the pursuit at Delium he, with Alcibiades and Laches, escaped by Registe, while others who would not follow him were overtaken and slain. This caused the dæmon of Socrates to be much talked of in Athens.

We may now dismiss these speculations, and come back to Socrates, and learn from himself what he understood and intended us to understand by his Dæmon or Dæmonion.

But here again we are brought to a standstill. We cannot interrogate Socrates himself. We can only get to him by hearsay. Between him and us stand Xenophon and Plato. It is, after all, Xenophon and Plato, not Socrates, who speak to us. Worse than this, Xenophon and Plato do not agree in what they tell us; and, worst of all, what they tell us evidently takes form and colour from their own minds. . . . . The Socrates of Xenophon stands out clear, hard, definite; a matter of fact description, a photograph with few after-touches, with little sense that anything needs explanation, or could have any meaning but the letter of the text. The Socrates of Plato comes to us through the prisms of his marvellous imagination; so as to create a misgiving whether it be a conscientious likeness or a portrait by the hand of an artist and a friend, too creative and too fond to be faithful. Nevertheless, we are reduced to those two biographers. They are the only full and trustworthy witnesses in close personal contact with the man whom they describe.

We will endeavour, then, to ascertain what they understood by the dæmon of Socrates. This will at least give the best approximation to what Socrates understood by it himself.

In order to do this, we will first take down their evidence as they gave it, and next compare the two testimonies; and, lastly, make an estimate of their differences. When this is done we may use our own criticism: for it is one thing to ascertain what Socrates may have understood, it is another to ascertain what we may understand by the psychological facts narrated by him or by them. It is not to be too hastily assumed that Socrates was an adequate interpreter of the internal facts even of his own mind. It is not unreasonable to believe that the philosophical and other profounder experience of two thousand years may have enabled us more truly than he could to analyze and to appreciate the facts and phenomena of moral and mental philosophy. The heart has beat, and the blood has circulated, from the beginning of time; yet we take the physiology of Harvey as to the blood, rather than that of Hippocrates. The Ethics even of Aristotle are, in analytical depth and precise delineation, conspicuously

* Plutarch, De Genio Socratis, sect. ix.

in advance of the method and teaching of Socrates. In this the disciple is above his master, and we may be above both.

Let us begin then with Xenophon. The chief passages in which he describes the Dæmonion are as follows:—

"It was in the mouths of men that Socrates declared that the Deity, or Dæmonion (τὸ δαιμόνιον) made things known to him, or gave him signs by which to know them."

"He used to say that the Dæmonion signified (things) to him; and that he often advised those who were with him to do some things, and not to do others, as the Dæmonion forewarned him."*

"For he thought that the Gods (τοὺς θεοὺς) had care of men in a way unlike that which most men imagine: for they suppose that the Gods know indeed some things, and do not know others. But Socrates believed the Gods to know all things; whatsoever things are said, or done, or purposed in secret; and that they are everywhere present: and that they make known human things to men."†

"When Hermogenes sorrowfully upbraided him for not defending himself more elaborately, and for even provoking his judges against him, Socrates answered: Of a truth, Hermogenes, when I set to work to think out my defence before the judges the Dæmonion hindered me.'"‡

Finally, Xenophon says of him that he was "so pious that he would do nothing without the counsel of the Gods."§

Such then is the evidence of Xenophon: upon which these remarks may be made.

1. That Xenophon carefully distinguishes between the Dæmon of Socrates, which he calls τὸ δαιμόνιον, and the Gods, whom he calls τοὺς θεοὺς.
2. That he describes the Dæmonion as showing beforehand what things are to be done, and what not to be done: that is to say, that the action of this monitor was both to enjoin and to forbid.
3. That he refers this admonition and direction to the Gods, without whose counsel (γνώμη) Socrates would never act.
4. That nothing in Xenophon is to be found which invests the Dæmonion with personality, or with any other character than that of a divine influence or agency, or a counsel or direction of the Gods acting upon the reason of man.

We will now proceed to our other witness.

The chief passages of Plato bearing on the Dæmonion are those which we have already quoted from the Apology. They need not be repeated.

To these may be added what follows.

In the Euthydemus, Socrates says: "I happened to be providentially sitting alone in the place where you saw me, in the dressing room (of the Lycæum), and I had in my mind to be gone. When I got up, the accustomed sign, the Dæmonion, came; I therefore again sat down."‖ Soon after came Euthydemus and his companions.

Again, in the Phædrus: "When I was about to cross the river, the Dæmonion, the accustomed sign, came, which restrains me when I am about to do anything: and I seemed to hear a certain voice, which did not suffer me to proceed until I should have expiated myself, as having in some way offended against God."¶

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* Xenoph. Mem. lib. i. c. 1, s. 1. Oxon. 1785. † Ibid. s. 4.
‡ Ibid. lib. iv. c. 8, s. 3. § Ibid. lib. iv. c. 8, s. 4.
‖ Euthydemus, 273, vol. vi. p. 80. Ed. Stallbaum, Gothæ, 1836.
¶ Phædrus, 242, vol. iv. 72.

And in the Alcibiades, he says: "The cause of this was nothing human, but a certain divine hindrance, the power of which you shall hereafter hear; but now, as it no longer hinders me, I am therefore come, and I am in good hope that for the future it will not hinder me." *

In the Theætetus, he says: "The Dæmonion which comes to me hinders my intercourse with some, and not with others."†

And in the Republic: "It is not worth while to speak of the divine sign which comes to me: whether it have occurred to any other or not."‡

I do not quote from the Theages, in which there is much on the point, for two reasons. First, doubts have been raised as to its authenticity: and secondly, the statements contained in it may be found also in other dialogues of which there is no doubt.

Now in these passages we have the following points:—

1. That the Dæmonion is spoken of as *θεῖον τί, δαιμόνιον, σημεῖον, φωνή*, and *εἰωθυῖα τοῦ δαιμονίου μαντική*; something divine, something of the Deity, a sign, a voice, the accustomed divination or oracle of the Deity. It is evident, therefore, that Plato represents it as an agency, or a voice, not as an agent or a person; and if the agent or person from whom this agency or voice proceed be sought for, it is to be found in God or in the Gods.

2. That the function of this agency or voice was to check, to hinder, and to restrain, not to suggest or to prompt to any line of action.

3. That it manifested itself in such apparently fortuitous events as the hindering the departure of Socrates from the Lyceum till Euthydemus came; and *πάνυ ἐπὶ σμικροῖς*, even in the least things: that is to say, its function was to forewarn or to check in matters not so much of right and wrong as of safety, or of expediency, or of good fortune.

Comparing these two testimonies of Xenophon and Plato we find—

1. That they agree as to the impersonal nature of the Dæmonion. The terms used by them signify at the utmost a divine agency or a divine voice; they do not signify the presence or attendance of a divine person, or of a familiar spirit.

It is, perhaps, not wonderful that some of the ancients should have so misunderstood their language, and that Socrates should have been accused of introducing new deities. The same charge was in like manner made against the Apostle at Athens, because he preached the Resurrection, *τὴν, ἀνάστασιν*. Nevertheless Cicero understood Plato's language, and translated the Dæmonion by "*divinum aliquid.*"

It is to be borne in mind that both Xenophon and Plato speak, not as we do, of the *Δαίμων* of Socrates, but of the *δαιμόνιον*. They never speak of the *δαιμόνιον* as *θεός*; but observe strictly the known distinction between these terms. *Δαιμόνιον* signifies the abstract or neuter idea of Divine power, the Deity, or the Divinity. As Aristotle says, it implies *ἢ Θεὸς ἢ Θεοῦ ἔργον*,§ either the presence or the power, not of a *δαίμων* or inferior divinity, but of God. *Δαίμων* is so far used convertibly with *Θεός*, that it is sometimes used for *Θεός*, but *Θεός* is never used for *Δαίμων*. *Θεὸς* and *Δαίμων* are sometimes used together; but *Δαίμων* signifies a divinity of lower rank.

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* Alcibiades, 103, vol. v. 221.
† Theætetus, 151, vol. viii. 71.
‡ De Republica, lib. vi. a. 496.
§ Arist. Rhet. ii. xxiii. 8.

In the Apology, Socrates tells his judges that when he was coming out of his house, τὸ τοῦ θεοῦ σημεῖον, the sign of the God, distinguishing the agency from the person, hindered him. Xenophon also makes Euthydemus say that the Gods showed a special friendship to Socrates. And again he says, speaking of voices, signs, and divinations, that by these things, τοὺς θεοὺς σημαίνειν, the Gods signified things to man. The δαιμόνιον was one of those signs: impersonal, derived from a divine agent.

2. That they disagree as to the function or office of the Dæmonion. Xenophon ascribes to it the twofold office of suggestion and restraint. Plato ascribes to it expressly that of restraint only. Plutarch agrees with Xenophon, and describes its office as either restraining or enjoining; κωλύων, ἢ κελεύων.*

3. That they further disagree, inasmuch as Xenophon recognizes the action of the Dæmonion in matters of right or wrong, as well as in matters of expediency; whereas Plato seems to restrict it to the latter.

The sum of the evidence, therefore, may be thus stated. Socrates believed himself to be assisted from his childhood by a divine agency, whereby he was forewarned and guided in matters of his own personal conduct, both towards himself, as in his escape after the defeat at Delium, and his waiting in the Lyceum; and towards others, as in judging what disciples to receive or to reject, and in his whole mission as cross-examiner of his fellow-countrymen.

Such is the judgment of ancient writers. I will quote only a few of the many modern critics on this subject.

Bishop Thirlwall says: "Socrates, who was used to reflect profoundly on the state of his own mind, had, it seems, gradually become convinced that he was favoured by the Gods with an inward sign, which he described as a voice."†

In like manner, Mr. Grote says: "We have also to note that marked feature in the character of Socrates, the standing upon his own individual reason, and measure of good and evil; nay, even perhaps his confidence in it so far as to believe in a divine voice informing and moving him."‡ Mr. Grote further refers in a note to a curious passage from the Life of Coriolanus by Plutarch, where he says that the Gods do not infuse into men new volitions; but they work upon the principle of association in the mind, suggesting ideas which conduct to the appropriate volitions. Plutarch's words are—"Not infusing the motive powers, but the ideas which call those motive powers into activity; not making the act involuntary by constraint, but giving an outset to the will, and inspiring it with courage and hope."

According to both these estimates it would appear that the groundwork of this divine action, as Socrates believed it to be, was the intellectual and moral activity of his own mind.

Zeller, in his work on Socrates and the Socratic Schools, gives his estimate of the Dæmonion in the following words:—

"The δαιμόνιον is therefore an internal oracle, and as such it is by Xenophon and Plato included under the more general notion of divination, and placed on a par with divination by sacrifice and the flight of birds. In attempting to bring

* De Genio Socratis, sect. xi. † Hist. of Greece, vol. iv. p. 290.
‡ Grote's Plato, vol. i. p. 295.

this inward revelation of Socrates into harmony with the facts of psychology, it may be laid down in the first place that the *dæmonium* must not be confounded with the voice of conscience, as many ancient and modern critics have done." . . . . "The δαιμόνιον has nothing to do with the universal moral standard, which, according to Socrates, is a matter for pure intelligence to determine." . . . . "For these [that is, actions in prospect] Socrates either has recourse to μαντική in general, or to his δαιμόνιον, leaving moral conduct to be determined by clear knowledge." *

We are here approaching to the estimate which appears to me to be both adequate and true.

Mr. Riddell, of Balliol, after carefully analyzing the evidence we have hitherto examined, says :—

"If, then, declining Socrates' account, we are disposed to refer the phenomenon to ordinary psychological causes, we can do so satisfactorily, provided we confine our attention to Xenophon's account alone. All Xenophon's notices of it encourage the view that it was a quick exercise of a judgment, informed by knowledge of the subject, trained by experience, and inferring from cause to effect, without consciousness of the process. In a mind so purified by temperance and self-knowledge, so single of purpose, and unperturbed by lower aims, endowed with such powerful natural faculties, especially those of observation and of causality, the ability to forecast and forejudge might become almost an immediate sense.

"As to the reconcilement of authorities, when Plato makes Socrates say, ἀεί ἀποτρέπει με, he describes it by its most perceptible act. For its coincidence with an existing purpose would be superfluous and little noticeable. . . . . The voice was no impulse: it did not speak to the will, but had a critical or reflexive function."

Mr. Riddell goes on to say, that the δαιμόνιον was "an unanalyzed act of judgment;" that it was κριτική, not ἐπιτακτική, that it was Socrates' substitute for μαντική, and that, where men are wont to have recourse to external preternatural aids, Socrates went by a guide within himself: that to this guide he, in all good faith, gave a religious name. "His mental acts, so far as he could unravel them, were his own, were human: beyond his ken, they were divine; and what really was of the nature of an immediate critical sense, seemed to him an immediate inspiration."†

This appears to me to be in outline an explanation both true and adequate.

If I were asked to add my own judgment as to what the Dæmon of Socrates was, in the estimate of Socrates himself, I should answer as follows :—

It was a belief of a divine assistance, granted, as he says, to all men in some things, and in some special circumstances; of which most men are hardly, if at all, conscious: but in his case it was consciously recognized from his childhood, and it acted upon him in and through the intellectual and moral operations of his own mind: so that he ascribed to the action of the Dæmonion much that was undoubtedly the normal activity of his own intellectual and moral state.

Such, I think, Socrates believed it to be.

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* Zeller's Socrates and the Socratic Schools, pp. 76–78. Longman, 1868.

† Riddell's Apology of Plato, pp. 105–8. Oxford, 1867.

If, however, I were asked what we may believe it to have been, I would answer:—

1. That, holding altogether with Lord Bacon, when he says in his 'Essay on Atheism,' "I had rather believe all the fables of the Legend, and the Talmud, and the Alcoran, than that this universal frame is without a mind"—we may well believe in a divine providence surrounding the life, and a divine action present to the mind, of any man who, according to the testimony of one that knew him by closest intimacy, was "so pious as never to do anything without the counsel of the Gods; so just as never to hurt anyone even in the lightest thing: but full of the greatest benefit to all who conversed with him: so temperate as never to prefer what was pleasant to what was best: so prudent as never to err in discerning the better from the worse; and never to need the judgment of any other in this discernment, being sufficient in himself."* That a divine providence and a divine help are over the whole intellectual and moral world, is an axiomatic truth in the relation of God to man: that they may be looked for in a special degree in just and prudent men, follows as a corollary from that axiom. But as this lies beyond our analysis, we will confine ourselves to the subject as a matter of psychology.

I will therefore add a further proposition, namely:—

2. That the statements of Xenophon and Plato may be, not only harmonized, but brought under the same psychological explanation, resting on the laws of the speculative and the practical reason. It would indeed be too narrow an explanation, as Zeller objects, to refer the signs of this monitor to the action of conscience alone; for conscience is only one office, or one function, of the reason of man. Nevertheless it is certain, that in a large part of that which Socrates referred to the Dæmonion, conscience was directly present and perceptibly in action. It is no objection to this to quote, as Zeller does, the declaration of Socrates that, "It is idle to consult the Gods about things which may be known by deliberation,"† or to say that Socrates "referred morals to the judgment of reason."‡ This is precisely the philosophical definition of conscience. Conscience is *dictamen rationis*. It is reason acting upon right and wrong with a view to practice: just as apprehension is the reason acting upon truth and falsehood with a view to science. In matter of speculative truth, as in physical science, geometry, and the like, the intellect acts without any response from the passions or affections of the moral nature. Aristotle in the 'Rhetoric' says that mathematics have no ethical character, but that the teaching of Socrates has.§ In matter of practical truth, the discernments of the intellect are followed by a response of the moral nature by way of approval or disapproval. But the *primary* judge is the reason, the response of the moral nature is *secondary*. It is, therefore, most true that morals are subject to the jurisdiction of

* Xenoph. Memorabilia, lib. iv. c. 1, s. 5. † Ibid. p. 78.
‡ Ibid. § Rhet. iii. 16, 8.

reason; but that does not prove that this monitor of Socrates was not in great part the action of conscience.

And here it may be well to make more clear and precise the philosophical definition of conscience.

Scientia is the simple knowledge of things by the reason. *Conscientia* is the self-knowledge of the reason or mind. "Nil conscire sibi, nulla pallescere culpa." Now this conscience in its *first* intention is consciousness, as we commonly call it. It extends over all the internal acts of the reason or mind, over the whole inner world of our personal identity. Conscience, in the *second* intention of common parlance, signifies the reason judging of moral action, and discerning of right from wrong, with an approval or disapproval of the moral sense following upon its decisions. Metaphysicians, therefore, distinguish the operations of conscience into two kinds, and speak of a *psychological* conscience, by which we reflect upon internal acts of the mind in general, and a *moral* conscience, by which we reflect upon and judge of the *ethical* character of actions, whether internal or external. And this conscience they again distinguish into *habitual* and *actual*. The *habitual* conscience is the permanent disposition of the mind in relation to its own moral *state*; the *actual* conscience is the exertion of its attention and judicial discernment on its own moral *acts*. The *habitual* conscience is *spontaneous*, and therefore unconscious; the *actual* conscience is reflex, *deliberate*, and therefore conscious.

Such is the language of scholastic philosophy; and this seems with great precision to account for the fact which Socrates appears to have observed in himself, and Plato has recorded; namely, that the Dæmonion seemed to act only by way of check and restraint. The spontaneous action of conscience was habitual and unperceived; the actual aroused attention and conscious effort.*

In saying, then, that this internal monitor of Socrates is the reason, including the conscience, I intend expressly to include what is here described as the psychological as well as the moral conscience, and also the distinction between the habitual or spontaneous, and the actual or deliberate conscience. The application of this will be further seen when we answer an objection which may be anticipated. This comprehensive view includes all the mental judgments, whether of expediency or of morality, that is, both of prudence and of rectitude.

As to the examples given by Xenophon and Plato, in almost every one of them may be traced a moral element and a moral discernment.

For instance: The decision of Socrates to keep out of politics, that he might better serve the public good. This is surely a high moral judgment, involving the noblest motives of relative duty. The discernment as to what disciples to retain, or what persons to receive back again among them after they had once left him, unless it were a blind and capricious act, which in Socrates is incredible, must have

* Prisco. Filosofia Speculativa, tom. i. pp. 208–10. Napolis, 1864.

been founded upon such a discernment of moral qualities and distinctions, both in his own character and in that of others, as to demand the exercise of the moral reason. When we say that one man is *simpatico* and another *antipatico*, we are exercising a moral sense and discrimination of an intimate and explicit sort; and this determines us in receiving or refusing to receive men to our confidence.

In deciding that he would not defend himself so as to escape death, the action of conscience, sustaining the highest aspirations and the noblest intrepidity, is clearly revealed.

I admit that in the escape after the flight and pursuit at Delium, and in the waiting in the Lycæum, and in the matter of Plutarch's pigs, there is to be discerned rather the activity of prudence than of conscience. But on the supposition that the monitor of Socrates was a mature and experienced reason, the action of both prudence and conscience would be alike included.

To this it may be objected, that Plato distinctly declares that the monitor of Socrates told him, not what things to do, but what things not to do; that inasmuch as conscience has a twofold office towards both good and evil, the Dæmonion could not be conscience.

But to this objection two answers may be made.

The first, that Xenophon and Plutarch directly say that the Dæmonion both enjoined and forbade, that is, pronounced for or against certain lines of action.

The other answer has been anticipated by the statements of our Scholastic Philosophy. It has been shown that the action of conscience, when it suggests or approves anything, is less preceptible than when it disapproves or forbids. This may be seen by analogies. We are insensible of our continuous respiration, but distinctly sensible of the act of holding the breath; it is an *actus imperatus* requiring a conscious exertion of the will. Again, in walking, we are unconscious of the momentum of our pace, but conscious of any hindrance, and even of the act of stopping. The moral reason or conscience is always in activity, but with little or no reflex action upon itself, until something offends it. We are then conscious of a change of attitude, and of a recoil. For instance, the reason and conscience of Socrates permitted him freely to mix among men to cross-examine them, but not to enter into politics. In the former, he followed his own spontaneous inclination; in the latter, he imposed a conscious restraint upon himself. This is what Aristotle describes as prudence, or Φρόνησις. He distinguishes it from science, as being an intellectual habit conversant with practical and contingent matter; and from intuition, as being of details rather than of principles. He says that Φρόνησις, or prudence, is an intellectual virtue conversant about moral action. And he ascribes to it a power of sight, which is so trained and perfected by experience as to discern with an intuitive rapidity what is right or expedient in practice. He says that prudent men have a faculty which men call (δεινότης) skill, or ability, or resource, "the nature of which is to do—and to do correctly—the

things which conduce to the end proposed. If this aim be good, the skill is praiseworthy; but if it be bad, it becomes craft." Wherefore Aristotle says, "we call prudent men skilful, and not crafty. But prudence is not the same as this faculty (*i. e.* δεινότης, or skill. But the habit of prudence grows upon this *eye*, as it were, of the soul."* This is a precise description of the prompt and provident intuition, a sort of ἀγχίνοια, and εὐβουλία, presence of mind, rapidity of counsel, with which Socrates discovered the useful, or the expedient in matters of practice. But the nature of this intellectual faculty is, in the main, distinctly moral; and belongs to the region of conscience, or the discernment of right and wrong.

This instinct or faculty of moral discernment is traceable throughout the whole history of the ancient world. St. Paul only affirms what all records of antiquity demonstrate in saying, "When the Gentiles which have not the law do by nature the things contained in the law, these, having not the law, are a law unto themselves, which show the works of the law written in their hearts, their conscience also bearing witness (συμμαρτυρούσης αὐτῶν τῆς συνειδησέως), and their thoughts the meanwhile accusing or else excusing one another."†

Once more: it may be objected that it is not for us to theorize as to what Socrates ought to have understood of his own inward life, but to take things as he expressed them.

To this I have already by anticipation made one sufficient answer. But I will add another. Socrates refused to be classed with the philosophers or teachers of Athens. He delivered no system of philosophy. He framed to himself no moral or mental science. He found philosophy in the hands of Physicists, or physical theorists, and Sophists. He thought the Physicists to be vainly curious, if not impious, in trying to discover what the Gods kept secret: he thought the Sophists to be venal, superficial, and immoral. He was the founder, not of a new philosophy, but of a new era in philosophy. He extricated the conceptions of God and of morality from the region and philosophy of matter, and set them in the sphere of mind. He brought down philosophy, as Cicero says, from heaven to earth, to the market-place, and the streets, and the homes and the hearts of men. He cross-examined every man he met with, politicians, philosophers, rhetoricians, painters, private citizens, artisans: but he framed no system, and laid down no theories; he made no analysis of the human mind. Lord Bacon is said to have created a Novum Organum in philosophy by questioning Nature. This Socrates certainly did by questioning man. His method was one of universal questioning, whereby he heaped up materials for his disciples, one of whom afterwards gave to them a scientific order and precision of expression which has formed the imperishable basis of mental and moral philosophy to this day. The Ethics of Aristotle analyze, lay out, distinguish, and define the

* Arist. Eth. N. L. vi. xii. τῷ ὄμματι τούτῳ · γίνεται τῆς ψυχῆς.

† Rom. ii. 14, 15.

intellectual and moral processes of the human mind—modern metaphysicians must bear with me—with a truth which has never been surpassed. What Socrates felt, Aristotle has fixed by exact analysis. The character of Socrates is the φρόνιμος of Aristotle, the prudent man; but prudence is etymologically and essentially far-seeing,* the perfection of the moral reason. "All men," he says, "seem to testify that such a habit which is according to prudence is virtue. But it is necessary to make a slight difference, for virtue is not only a habit according to right reason, but inseparably joined with right reason; and prudence is the same as right reason on these subjects. Socrates therefore," Aristotle says, "thought the virtues to be reasons or rational habits, for he thought them all to be sciences, but we think them to be intellectual habits joined with reason. It is clear, however, from what has been said, that it is impossible for a man to be properly virtuous without prudence, or to be prudent without moral virtue."† Aristotle seems to me to give in this passage the psychological analysis of the intuition and providence with which Socrates was eminently endowed. His prudence or φρόνησις constituted the αὐτάρκεια, or self-dependence of reason in all questions of morality, of which Xenophon speaks.

> Nullum numen abest si sit Prudentia, nos te
> Nos facimus, Fortuna, deam cœloque locamus.‡

The prudence of Socrates was his own moral state, and yet *non sine Numine*, for we may well believe that to him was granted no common share in the "Light that lighteth every man that cometh into this world."

In saying this, I am not rejecting the supposition that the particular providence which never suffers even a sparrow to fall to the ground without its Creator's will, may have in a special way encompassed the life of a man who witnessed in a corrupt world to the lights of nature and to the laws of right. In the midst of an intellectual frivolity and a moral degradation never surpassed in the history of mankind, made all the guiltier by reason of the refined culture and luxurious civilization of Athens, Socrates bore witness, until seventy years of age, to the supremacy of prudence, justice, fortitude, and temperance, the four perfections of man in the order of nature.

Whether the estimate I have given of the Dæmonion of Socrates be true or not, the inquiry in which we have been engaged is manifestly not a barren speculation. It sets before us a great moral example, it teaches us a great moral law, necessary to men at all times, vital to us in these declining days. I mean, that there is no way for men to attain their true dignity, nor to serve their age and

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* Prudens futuri temporis exitum
Caliginosa nocte premit Deus.—*Hor. Od.* iii. 29.

† Eth. N. L. vi. xiii. ‡ Juvenal, lib. iv. sat. x. 365–6.

country, but to be upright in conscience, and even at the cost of life to be both in public and private duty prudent and temperate, just and brave. It tells us with a thrilling human voice, and in the accents of our common humanity, that man's supreme rule of right is the moral reason or conscience; that the cultivation of the mere intellect, while the moral life and powers lie fallow, is the work of sophists, deceivers, or deceived, or both; that the education of man is his moral formation; that intellectual culture without moral goodness is a wildfire and a pestilence which makes havoc of men and states; that knowledge is virtue, and virtue knowledge; for that, unless we would maim and mutilate our being, the intellectual and moral powers of man must be simultaneously and equably unfolded and matured. These are axioms of the moral life; vital, I say, at all times and in all lands, but nowhere more in season and more wholesome than to us who, in the sudden growth of a vast maritime empire, splendid and unstable from its very greatness, in the refinements of luxury, and the inundation of a stupendous prosperity, seem to be developing some of the moral and intellectual evils which went before the fall of imperial Athens;—political factions, licentious freedom, sophistical education, a relaxation of moral and religious traditions, a growing scepticism, an unstable public opinion swayed to and fro by nameless hands, and by irresponsible voices. In such a public state Socrates lived and died, bequeathing to us this lesson—that Conscience is the Voice of God.

WEEKLY EVENING MEETING,

Friday, February 2, 1872.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Professor Tyndall, LL.D. F.R.S.

On the Identity of Light and Radiant Heat.

Whether we regard its achievements in the past, or its promise and tendency in the future, all that we know of physical science—every bent and bias which we receive from its pursuit—tends to confirm the dictum of the poet regarding this universe:—

> "All are but parts of one stupendous whole,
> Whose body Nature is."*

If I halt here, and omit the next clause of the couplet, it is not because physical science has arrived at any conclusion hostile to that

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* "All are but parts of one stupendous whole,
Whose body Nature is, and God the soul."
Pope's *Essay on Man*, Epistle I., line 267.

clause, at all events in its profoundest signification, but simply because what the poet goes on to affirm lies outside the sphere of science. We, as physical students, have to do with "Nature" only, and our view of nature could not be more happily expressed than by the figure employed by the poet. For our vocation, and the delight and discipline that it confers, do not consist in the registration of unrelated facts and phenomena; but in the searching out and discovery of relationship in a system, whose parts we hold to be as closely and definitely related to each other as are the various organs and functions of the living body itself.

It was this spirit of search, this capacity and desire, developed amid natural agencies, to detect the lines of connection between these agencies, that gave for a time such keen interest to the discussion, whether light and heat were essentially different things, or whether a substantial identity subsisted between them. It is not so very many years since that most excellent experimenter and philosophical inquirer, Melloni, isolated from a solar beam a brilliant light, and finding it incompetent to affect his most sensitive thermoscopic apparatus, concluded that light and heat were essentially distinct. But in drawing this conclusion, Melloni forgot that he was implicitly dealing with an instrument of almost infinitely greater delicacy than his thermoscopic apparatus; he forgot that the human eye, and the consciousness connected with the eye, are capable of being vividly excited by an amount of force which when translated into heat might defy all the thermometers in the world to detect it. Melloni himself subsequently modified his conclusion.

It is not so very long since the late Principal Forbes was eagerly engaged in establishing the important point that radiant heat, like light, is capable of being polarized. Since that time Knoblauch, Foucault, Fizeau, and Seebeck have applied their refined experimental skill to this question of identity; and those excellent investigators De la Provostaye and Desains, pushed the analogy between light and heat so far as to prove that the magnetization of a ray of light, in Faraday's sense of the term, has its parallel in the magnetization of a ray of heat.

It was, however, in their private cabinets that these experimenters obtained their results, which were in most cases so small, as to require attention on the part of a skilled observer to detect them. But science grows; and our experimental means augment as our knowledge expands. Recent discoveries and improvements will, I trust, enable me to make evident to you, to-night, effects which have been hitherto confined to far more limited circles; some of which indeed have only been seen by the observers who first noticed and described them. And if those accidents which often hold sway over lecture-experiments of a delicate character should prove favourable, we may be able to push the subject a hair's breadth beyond the limits which observation has hitherto assigned to it.

Heat is presented to us in two aspects: sometimes associated with

ordinary matter, through which it creeps by the process of conduction; sometimes not associated with ordinary matter, but, like light, flying through space with immense velocity. In this latter form it is called *radiant heat*. Radiant heat obviously and palpably comes to us from the Sun, but here it is entangled with light. Let me, in the first place, endeavour to unravel this entanglement.

When white light is refracted, it is unravelled and the spectrum is produced. A spectrum of the electric light was thrown upon a screen; and red, green, and black ribbons about an inch wide were successively moved along it. The red placed in the red light appeared a brilliant red; when moved into the green it became black. In like manner the green ribbon moved from the green, where it shone vividly green, into the red, became an intense black. The black ribbon was black in every part of the spectrum.

Now the red ribbon is not heated in red, and the green is not heated in green; but red is heated in green, and green in red. We have heating only where we have absorption; and the heat generated is the equivalent of the light absorbed. Black absorbs all the rays of light, hence, indeed, its blackness; and if it could speak, it might tell us the warmth of every colour. But warmth exists outside the colours. Beyond the red, where nothing is seen, the force acting on the retina is far greater than when the eye is plunged in the red. The objective here is entirely out of proportion to the subjective.

The existence of this heat was thus proved. All the colours but the red were cut off by a red glass, and with a diaphragm having a circular opening, a well-defined red circle was produced. This was refracted by a prism, still remaining a circle. A thermo-pile with its face towards the lamp was then caused to approach the path of the beam. It would have been seen by its shadow on the screen if the light had been at all invaded; but with a considerable interval between the pile and the light, a large deflection of the galvanometer testified to the presence of heat beyond the luminous circle. An opaque solution* was substituted for the red glass. A circle remained, but it was an invisible circle of radiant heat instead of a circle of light, and the needle of the galvanometer did not fall, though the visible image had vanished.

Thus, as regards refraction, we have radiant heat behaving like light. And now for reflexion. A horizontal beam of light was reflected upwards by a plane mirror, and when the light was cut off by the introduction of the opaque cell, a powerful beam of reflected heat was proved still to remain. The luminous beam was then *totally reflected* by a prism to a horizontal direction; the light was again cut off, and a powerful deflection of the galvanometer needle was obtained by the residual heat-beam. Thus, in respect to common and total reflexion the behaviour of light and heat is the same.

* Iodine in bisulphide of carbon.

The action of lenses on light and heat was then demonstrated, the invisible heat-rays being brought to a focus as readily as the rays of light.

A beam of light was then made to strike a concave mirror, and at the focus, which was strikingly visible in the dust of the room, the thermo-pile was placed, having its face covered. The light being cut off by the dark cell, and the covering screen drawn away, the needle of the galvanometer at once flew to its stops.

Double refraction by Iceland spar was next described and explained. It was illustrated by passing through the spar a circular beam of light, which, on the screen, gave two images. The places on the screen where these two images fell were marked, and the light was cut off by the iodine cell. On introducing the thermo-pile with its face towards the lamp, when it occupied the position of either light-image, a deflection of the needle was obtained. Of the two images, one is the ordinary, the other the extraordinary. Is the same true of the heat? Placing the pile in the place of the ordinary image, cutting off the light, and turning the spar, the deflection of the needle remained unchanged; but when the spar was turned round, while the pile occupied the place of the extraordinary image, the needle instantly fell. Why? Removing the dark cell and rotating the spar, the extraordinary light-image was seen to rotate round the ordinary one, which remained fixed. The heat-beam did the same and thus quitted the pile. Here then we prove that the heat-beam also has its ordinary and extraordinary image. This, it was believed, was the first time the effect had been obtained with purely invisible heat. Knoblauch had demonstrated the double refraction of heat, using the total beam, luminous and non-luminous, of the Sun.

Some of the phenomena of polarization were next touched on. Light is propagated by the undulations of an etherial medium, the direction of vibration being perpendicular to the direction of propagation. A crystal of tourmaline has the property of quenching all vibrations except those which are parallel to the axis of the crystal; hence, a plate of tourmaline cut parallel to the axis will allow all vibrations in that direction to pass through it, but will stop all others. A beam of light which has passed through one plate of tourmaline is therefore unable to pass through another placed transversely to it, whereas, if the axes are parallel, the light is but little dimmed by the second plate. The black space due to the superposition of the crossed plates of tourmaline was shown, as also the abolition of the darkness by a thin film of mica introduced between the plates.

A beam with all its vibrations reduced to the same plane is called a beam of *plane polarized light.*

The two beams emergent from double-refracting spar are thus polarized. Nicol got rid of one. He cut a parallelopiped of spar into two by a very oblique section, polished the two surfaces, and united them by Canada balsam. The ordinary or more powerfully refracted ray, at the surface of the balsam is, in consequence of its obliquity,

totally reflected, and the extraordinary ray passes on alone. In this way we obtain an intense beam of polarized light.

A beam of light was sent through two large Nicol prisms, and shown to be entirely extinguished when the principal sections of the prisms crossed each other. The introduction of a plate of mica between them caused, as in the case of the crossed tourmalines, the instant reappearance of the light. The opaque cell was then placed in front of the lamp, all visible rays being thus intercepted. The thermo-pile was next placed so as to receive the beam after leaving the second Nicol prism. Causing one of the crossed prisms to rotate, a path was opened for the heat exactly as for the light, the deflection of the needle speedily bearing witness to the fact. The prisms being again crossed, the heat-beam was again quenched; but as in the case of light, the introduction of a piece of mica restored the heat and caused a large deflection of the galvanometer.

Faraday's great experiment was next performed. A beam of light, polarized by one Nicol's prism, was made to pass through a piece of heavy glass placed between the perforated poles of an electro-magnet, and afterwards through another Nicol, so placed that the beam was extinguished. When the magnet was excited the plane of polarization was caused to rotate and a luminous image flashed instantly out upon the scene.

The effect of magnetization is greatly augmented by adopting the device of MM. De la Provostaye and Desains, of causing the principal sections of the Nicol's prism to enclose, not a right angle, but an angle of 45°. This was done, the heat falling on the pile being neutralized by the method of compensation. On sending a current round the magnet a considerable deflection of the needle was obtained, the direction of the deflection depending on that of the magnetizing current.

De la Provostaye and Desains thus obtained with luminous solar heat a deflection of two or three degrees. With the iodine filter and the electric lamp a deflection equivalent to 150 of the lower degrees of the galvanometer was obtained from purely non-luminous heat.

[J. T.]

GENERAL MONTHLY MEETING,

Monday, February 5, 1872.

Sir Frederick Pollock, Bart. M.A. Vice-President, in the Chair.

Alexander Brodie, Esq.
John Cleghorn, Esq. F.R.G.S.
Edward John Gayer, Esq. M.R.C.S.
Arthur Edward Griffiths, Esq.
William Grogan, Esq.
The Hon. Frederick H. North.
Samuel Wagstaff Smith, Esq.
W. Soames, Esq.
Henry Virtue Tebbs, Esq.
Burney Yeo, M.D.
Henry Yool, Esq.

were *elected* Members of the Royal Institution.

The following Letter from the Emperor of Brazil to Dr. Bence Jones, the Honorary Secretary, was read :—

Sir,

In requesting you to offer my best thanks to the Members of the Royal Institution of Great Britain, for the mark of distinction that they have been good enough to confer upon me, I hope I may be permitted to say, in justice to other Brazilians, that I feel conscious the choice might have fallen on one more worthy than myself of the kindly feeling shown to me. The duties that my position impose upon me hardly allow me time to avail myself of the advantages of science, or properly to appreciate the merits of its expounders, but I can assure you that our Society may rely upon the zeal which I shall display in its cause, and the sincerity with which I shall ever subscribe myself,

Its constant admirer,

Paris, the 1st January, 1872. D. Pedro d'Alcantara.

The special thanks of the Members were returned for the following Donations to "the Fund for the Promotion of Experimental Researches" :—

Professor Tyndall (*3rd Donation*)	£30
Arthur Giles Puller, Esq. (*5th Donation*)	21

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

Antiquaries, Society of—Proceedings, Vol. V. No. 2. 8vo. 1871.
Asiatic Society of Bengal—Journal, Nos. 170, 171. 8vo. 1871.
Proceedings, 1871. Nos. 9, 10, 11. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. XXXII. Nos. 1-3. 1871-2.
Bavarian Academy of Sciences, Royal—Abhandlungen; Band X. Abtheilung 3. 4to. 1870.
Sitzungsberichte. 1871 : Heft 1. 8vo.
Annalen der Kön. Sternwarte bei München. Bände XVII. XVIII., und Supplement-bände IX. XI. 8vo. 1869-71.

Basel Naturforschende Gesellschaft—Verhandlungen: Theil V. Heft 3. 8vo. 1871.

British Architects, Royal Institute of—Sessional Papers, 1871–2. No. 3. 4to.
General Conference of Architects. 1871. 4to.

British Museum Trustees—Catalogue of Hemiptera Heteroptera. Part 4. 8vo. 1871.

Chemical Society—Journal for Dec. 1871. 8vo.

Church, Arthur H. Esq. M.R.I.—Catalogue of Pottery in the Collection of A. H. Church. 16mo. 1870.
Corinium Museum Guide. 16mo. 1871.

Comitato Geologico d'Italia—Bollettini. 1871. Nos. 11, 12. 8vo. Firenze.

Cox, Edward W. Esq. S.L. M.R.I. (the Author)—Spiritualism answered by Science. 8vo. 1871.

Dawson, J. W. LL.D. F.R.S. (the Author)—Report on the Geological Structure and Mineral Resources of Prince Edward Island. (L 15) 8vo. 1871.

Editors—Academy for Dec. 1871, Jan. 1872. 4to.
American Journal of Science for Dec. 1871, Jan. 1872. 8vo.
Artizan for Dec. 1871, Jan. 1872. 4to.
Athenæum for Dec. 1871, Jan. 1872. 4to.
Chemical News for Dec. 1871, Jan. 1872. 4to.
Engineer for Dec. 1871, Jan. 1872.
Horological Journal for Dec. 1871, Jan. 1872. 8vo.
Journal of Gas-Lighting for Dec. 1871, Jan. 1872. 4to.
Mechanics' Magazine for Dec. 1871, Jan. 1872. 8vo.
Nature for Dec. 1871, Jan. 1872. 4to.
Pharmaceutical Journal for Dec. 1871, Jan. 1872. 8vo.
Photographic News for Dec. 1871, Jan. 1872. 4to.

Franklin Institute—Journal, Nos. 550, 551, 552. 8vo. 1871.

Geological Institute, Imperial, Vienna—Jahrbuch, 1871. No. 3. 8vo.
Verhandlungen, 1871. No. II. 8vo.

Geological Society—Quarterly Journal, No. 109. 8vo. 1871.

Haughton, A. B. M.D. (the Author)—Laws of Vital Force in Health and Disease. 16mo. 1869.

Holland, Sir Henry, Bart. M.D. D.C.L. F.R.S. Pres. R.I. (the Author)—Recollections of Past Life. 12mo. 1872.

Institution of Civil Engineers—Address of T. Hawksley, Esq. the President. 8vo. 1872.

Lee, Robert, M.D. F.R.S. (the Author)—Treatise on Hysteria, &c. 8vo. 1871.

Linnean Society—Journal, No. 54. 8vo. 1871.
Proceedings, 1871–2. 8vo.
Transactions, Vol. XXVII. Part 4; Vol. XXVIII. Part 1. 4to. 1871–2.

London Institution—Journal, No. 10. 8vo. 1872.

Longmans & Co. Messrs.—Public School Atlas of Modern Geography. Edited by the Rev. George Butler. 4to. 1872.

Manchester Literary and Philosophical Society—Proceedings. Vol. XI. Nos. 4–8. 8vo. 1871–2.

Mechanical Engineers' Institution, Birmingham—Proceedings, 1871: July, Part 2. 8vo.

Newdigate, A. L. Esq. M.A. M.R.I.—Scales for the Ready Comparison of British and Metric Weights and Measures.

Odontological Society—Transactions. 8 vols. 8vo. 1856–71.

Preussische Akademie der Wissenschaften—Monatsberichte, Sept. Oct. 1871. 8vo.

Royal Society of London—Proceedings, No. 130. 8vo. 1871.

Société Hollandaise des Sciences—Natuurkundige Verhandelingen, 3me Série, Tome 1. Specieller Theil. 4to. 1871.
Archives Néerlandaises, Tome VI. 4, 5. 8vo. 1871.

Statistical Society—Journal, Vol. XXXIV. Part 4. 8vo. 1872.

Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, Dec. 1871, Jan. 1872. 8vo.

United Service Institution, Royal—Journal, No. 65. 8vo. 1871.
Watts, Henry, Esq. F.R.S. (the Author)—Dictionary of Chemistry—Supplement. 8vo. 1872.
Yorkshire Archæological and Topographical Association—Journal, Part 6. 8vo. 1871.

WEEKLY EVENING MEETING,

Friday, February 9, 1872.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Professor Humphry, F.R.S.

On Sleep.

The Professor first gave a brief account of some of the changes that take place in the tissues when their function is active, and explained that during this time a slight deterioration of structure takes place, which, affecting the voluntary system—the muscles and the hemispheres of the brain—causes the sense of tiring, and necessitates a period of rest for the restoration of the tissue to its former condition. In the case of the muscles this rest is provided for by periods, quickly alternating periods, of action and cessation of action. But in the case of the brain, the actions upon which consciousness, volition, &c., depend cannot be thus frequently suspended. Their continuance is needed for the safety of the body during long periods, through the whole day for instance; and longer periods are therefore required for repair. These are the periods of sleep.

He next took a cursory glance at the different parts of the nervous system, explaining that the upper regions of the brain are those which minister to consciousness and volition, the intellectual operations, &c. He showed that the functions of these regions not only can long be suspended without interfering with the action of the lower parts of the brain which are more immediately necessary to life, but that they are very easily suspended, slight causes, such as a jar or a shock, or an alteration in the blood current, being sufficient to stop the action of these parts, and deprive the person of consciousness. The spontaneous stoppage of their action, consequent on the slight deterioration of their structure from the continuance of their functions during the day, is the proximate cause of sleep during the night; and the periodic recurrence of sleep is in accordance with the periodicity observed in several of the nutritive functions, and indeed witnessed in many of the other operations of nature.

After observations upon the condition of the brain during sleep, the circumstances that conduce to sleep, the time that should be allotted to it, and other points, the Professor entered at some length

into the subject of dreams. These he regarded not, as has been supposed by some, to be a necessary attendant on, or feature of, sleep, but rather to be the result of an abnormal condition. In the natural state we should pass from wakefulness to complete unconsciousness, and *vice versâ*, quickly, almost instantaneously; and many persons habitually do so. But the transition period is sometimes prolonged, and stages are observable. The first thing that occurs is the lowering or cessation of that control over the mental processes which is the highest of our powers, the one requiring the greatest effort and the one most easily lost. In this condition the thoughts ramble unchecked, chase one another confusedly over the mental field, and give rise to all sorts of incongruities of the imagination. At the same time being unrestrained, they are excited, and evince efforts of memory and even of combination which in the regulated state of wakefulness they are quite incapable of. In this way the images of persons and places, of events and items of knowledge long forgotten in the ordinary state, are recalled with distinctness; and we fancy that new information has been acquired, when it is only forgotten facts that are recalled. He did not agree with the physiologists who conceive that dreaming depends upon an inequality in the condition of different parts of the brain, some being excited or wakeful, while others are quiescent or asleep. He rather took the view that all the parts of the cerebral hemispheres combine in each of the efforts of control, consciousness, memory, and other mental acts, that all suffer alike from these effects, alike need the restoring changes which take place in sleep, and together, *pari passu*, pass through the stages on the way to and from sleep in which dreaming, sleep-walking, &c., occur.

[G. M. H.]

WEEKLY EVENING MEETING,

Friday, February 16, 1872.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

John Hall Gladstone, Esq. Ph.D. F.R.S. F.C.S. M.R.I.

On the Crystallization of Silver, Gold, and other Metals.

There are few chemical experiments so well known as the growth of the "lead tree," a specimen of which is on the table, together with a "silver tree" that is said to have been made by the late Professor Faraday. These carry our minds back to the time of the alchemists, who called the first "arbor Saturni," and the second "arbor Dianæ;" and they may be looked upon as the types of a large number of

phenomena, in which the salt of one metal in solution is decomposed by some other metal. My assistant, Mr. Tribe, and myself have been lately examining these replacements, the metallic crystals which are thus produced, and the forces that act through the liquid.

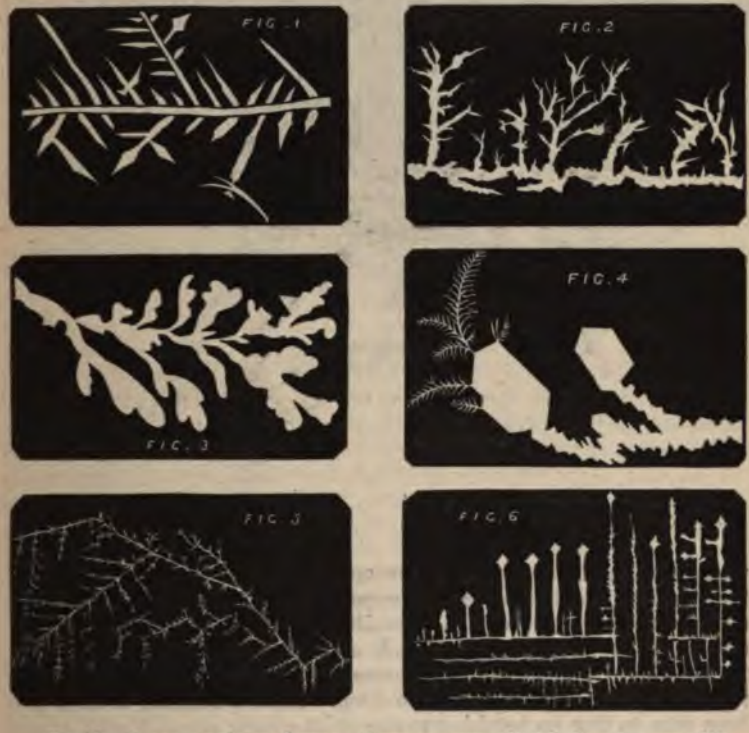
Our more special attention has been given to the mutual action of copper and nitrate of silver. If these two substances be brought into contact by the intervention of water, there grow upon the red metal what may be well called "trees," and though the analogy between crystals and plants is a very superficial one, yet the resemblances of external form are striking enough, and a nomenclature drawn from the garden seems the most expressive.

It is very beautiful to watch the growth of these silver crystals round a piece of copper under the microscope; a blue glass underneath adds to the effect, and they are best seen when they reflect a strong light thrown upon them. They may be also thrown upon a screen as opaque objects, but the beauty and lustre of their surface is in this way lost.

The crystals of silver thus produced differ both in colour and form, according to the strength of the solution. If it be very weak—say one per cent.—the copper is fringed with black bushes of the metal, which, in growing, change their colour to white without any alteration of crystalline form that can be detected by a powerful microscope. A stronger solution gives white crystals from the commencement, which frequently assume the appearance of fern-leaves; while the growth from a still stronger liquid reminds us rather of a furze bush. If the nitrate of silver amount to 15 per cent., or thereabouts, there occurs a steady advance of brilliantly white moss; and if the solution be saturated, or nearly so, say 40 per cent., this moss is very sturdy, often ending in solid crystalline knobs, or stretching out into the liquid as an arborescent fringe.

In all these cases, however, when the solution in front of the growing crystals has been somewhat exhausted, certain prominent or well-circumstanced crystals seem to monopolize the power, and to push forward through the remaining portions of the liquid. This gives rise to beautiful branches which assume a variety of graceful forms, which it is hopeless to attempt to pourtray by diagrams, but of which the subjoined figures give some of the more characteristic outlines greatly magnified. The weak solutions produce feathery crystals somewhat as in Fig. 1, consisting of a straight central stem from which grow on either side crystalline rays that terminate in a sharp point, and frequently become themselves the centre stems of a similar crystalline structure. In the outlying growth of a moderately strong solution the apparent regularity of the crystalline form is lost; the main stem is built up of a confused mass of hexagonal plates, while the side branches are an agglomeration of minute pointed crystals turning in every direction, and producing such jagged outlines as are drawn in Fig. 2. In stronger solutions still the branches lose every appearance of straightness, and they are built up of hexagonal plates so studded

with crystalline specks that the whole has the rounded appearance depicted in Fig. 3. The arborescent crystals that succeed the fringes from a saturated solution, are smaller in their foliage than the last, and end in little spherical or botryoidal knobs.



Beside these various forms, there occur all kinds of crystalline combinations, as, for instance, the spray sketched in Fig. 4, where the rough branches have terminated each in a large hexagonal plate, and the flowing past of a weakened solution has afterwards caused the growth of delicate fern-leaves. Often, too, a large expansion will take place in every direction, though joined to the parent stem by an almost invisible thread; or from the point of a long crystal there will branch out to right and left crescent-shaped structures, a process the commencement of which is seen in one of the side rays of Fig. 1. The last traces of silver in the liquid will frequently give rise to delicate crystalline filaments wandering over the surface of the glass, as in Fig. 5.

If a piece of zinc be placed in a solution of neutral terchloride of gold, containing about 9 per cent. of salt, there is an immediate out-

growth of black gold, which speedily changes to an advancing mass of yellow, or perhaps lilac metal in lichen-like forms, from which proceed beautiful fringes of yellow or black, ending generally in such arborescent forms as are represented in Fig. 7. As these branches

FIG. 7.

push into the yellow liquid, it becomes colourless even in advance of their points, and it frequently happens that yellow crystals of some salt shoot out in front of the crystallizing metal, which follows them and builds up its advancing fronds at their expense. This is shown in the figure. The gold will generally shoot its yellow branches rapidly round the margin of the drop. Such a running branch has been seen to stop on touching at one point a loose piece of gold, which immediately in its turn became active, and commenced to sprout on its farther side.

Copper salts give round nodules, which have no crystalline appearance when deposited from moderately weak solutions, but a very strong solution of the chloride—about 40 per cent.—yields with zinc first a black thick growth, then arborescent fringes of red metal, terminating in crystals of very appreciable size.

The fringes referred to in the case of these three metals are still more characteristically developed by bismuth. When a solution of terchloride of bismuth acts on zinc there is an immediate outgrowth of black fringes, such as Fig. 8, where they are seen on an illuminated field. These as they advance become more and more arborescent, and as the crystalline character develops itself more they change from black to grey. Sometimes bismuth presents itself in

botryoidal masses, but the tendency to form these fringes is very strong.

Fig. 8.

Chloride of antimony with zinc also gives these black fringes.

Lead salts yield crystals resembling those of silver; but leaves of irregular hexagonal plates prevail, and often grow to a large size.

A solution of acetate of thallium, containing 20 per cent. of salt, quickly gives a beautiful forest of thorny crystals.

Sulphate of cadmium gives rise to a small leaf-like growth on zinc; but a strong solution of the chloride produces an appearance of sticks covered with small spines or knobs.

The new metal indium is thrown down upon zinc in the form of thick white crystals. The deposition is promoted by touching the zinc with a piece of iron.

Tin gives beautiful results. If zinc be placed in a solution of stannous chloride it is quickly surrounded with an outgrowth of prolonged octohedra, and as these advance into the liquid it is easy to observe that the additions of new metal commence at the apex, and that the wave of chemical change proceeds down the lateral edge, occupying some seconds of time in depositing the new layer of material. Frequently, also, there is a luxuriant growth of large flat leaflets, or of symmetrical structures resembling fern-leaves, but with the fronds arranged at right angles, or combinations of these with octohedra, as shown in Fig. 6. These fern-leaves often begin of a dull grey colour; but, as they advance, suddenly change to a brilliant white.

The particular form of these crystalline growths depends, therefore, primarily, on the specific character of the metal; but this is greatly modified by the strength of the solution.

The forms assumed by native metals resemble those produced by this process of substitution. In some cases, indeed, it seems almost certain that the deposition of these minerals was effected in the same

way, as, for instance, the silver which occurs sometimes in tufts, sometimes in large crystals, on the native copper of the Lake Superior district. Gold is frequently found in cubes more or less rolled, but the leaf gold from Transylvania bears a striking likeness to the crystals that form in our laboratory experiments. Silver is often found native as twisted hairs or wires of metal—a form that never occurs in the decomposition of its nitrate by copper, but which can be artificially produced in another way.

There has been noticed a singular tendency in old silver ornaments and coins to become crystalline and friable. Here is an ancient fibula from the island of Cyprus, supposed to be at least 1500 years old, which, through the greater portion of its substance, presents a fracture something like that of cast iron, and its specific gravity has been reduced in round numbers from 10 to 9. It contains a little copper. This property of certain metals, or their alloys, to change in condition and in volume, is worthy the attention of those whose duty it is to make our standards. Experiments should be instituted for the purpose of learning what metals or combinations of metals are least subject to this secular change.

These metallic crystals are Nature's first attempt at building. The material is the simplest possible—in fact, what chemists look upon as elementary. But how is the building carried on? What are the tools employed? Where are the bearers of burdens that bring the prepared pieces and lay them together according to the plan of the Great Architect? We must try to imagine what is taking place in the transparent solution. The silver, of course, existed at first in combination with the nitric element, and for every particle of silver deposited on the growing tree, an equivalent particle of copper is dissolved from the surface of the plate. The nitric element never ceases to be in combination with a metal, but is transferred from the one metal to the other. On the "Polarization Theory," the positive and negative elements of the salt constantly change places and enter into fresh combinations, one consequence of which would be a gradual passage of the nitric element from the growing silver to the copper plate. This actually takes place, and there is a diminution of the salt at the ends of the silver branches, giving rise to an upward current, and a condensation of nitrate of copper against the copper plate, which gives rise to a strong downward current. These two currents are seen in every reaction of this nature. In the case of silver and copper, however, it has been proved that the crowding of the salt towards the copper plate is more rapid than would follow from the usual polarization theory. The instrument employed for determining this point was a divided cell in which two plates, one of silver and the other of copper, connected together by a wire, are immersed each in a solution of its own nitrate, contained in each division of the cell, and separated from one another merely by parchment paper. The crystals of silver deposited on the silver plate in this experiment are very brilliant.

There are other indications of the liquid being put into a special condition by the presence of the two metals which touch one another. Thus zinc alone is incapable of decomposing pure water; but if copper or platinum be deposited on the zinc in such a manner that the water can have free access to the junction of the two metals, a decomposition is effected; oxide of zinc is formed, and hydrogen gas is evolved. At the ordinary temperature the bubbles of gas rise slowly through the liquid, but if the whole be placed in a flask and heated, pure hydrogen is given off in large quantity. We have also found that iron or lead similarly brought into intimate union with a more electro-negative metal, and well washed, will decompose pure water.

As might be expected, the action of magnesium on water may be greatly enhanced by this method; and a pretty and instructive experiment may be made by placing a coil of magnesium in pure water at the ordinary temperature, when there will be scarcely any effect visible, and then adding a solution of sulphate of copper. The magnesium is instantly covered with a growth of the other metal, and at the same time the liquid seems to boil with the rapid evolution of hydrogen bubbles from the decomposed water.

When, however, the force of the two metals in contact has to traverse a layer of water, the resistance offered by the fluid prevents its decomposition. This must also be an important element in the decomposition of a metallic salt dissolved in water, and in fact we have found that the addition of some neutral salt, such as nitrate of potassium, increases the action—apparently by diminishing the resistance of the liquid. If, too, we increase the quantity of the dissolved metallic salt, we get more than a proportional increase of deposited metal. Thus, in an experiment made with the different strengths of nitrate of silver on the table, the following results were obtained in ten minutes, all the circumstances being the same except the strength of the solution:—

1	per cent.	solution	dissolved	·025	grm. copper.
2	,,	,,	,,	·078	,,
4	,,	,,	,,	·224	,,

In fact it has been found that in solutions not exceeding 5 per cent., twice the amount of nitrate of silver dissolved in water gives three times the amount of chemical action; and this is true with other metals also in weak solution. It may be that this is not the precise expression of a physical law, but it agrees at least very closely with the results of experiment.

The power arising from this action of two metals on a binary liquid may be carried to a distance and produce similar decompositions there. This is ordinary electrolysis. Metals have often been crystallized from their solutions in this way, and Mr. Braham has made excellent preparations of crystalline silver, gold, copper, tin, platinum, &c., by using poles of the same metal as that intended to

be deposited upon them. The forms thus obtained are precisely analogous to those produced by the simple immersion of one metal into the soluble salt of another, and illustrate still further the essential unity of the force that originates the two classes of phenomena.

[J. H. G.]

WEEKLY EVENING MEETING,

Friday, February 23, 1872.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Henry Leslie, Esq.

On the Social Influence of Music.

In these days of railways and telegraphs, and of that general high pressure to which they have brought us, it is necessary to utilize every calming and soothing influence, and to find rational mental employment for the people as recreation after their hours of work. Such a course cannot fail to be productive of increased strength to the ties of social and family life, and consequently of powerful good to the national life.

Numerous examples taken from the various strata of society which form the national life of the kingdom are cited to show how great the social influence of music is at the present time, both as a means of education and recreation, and how refining and elevating are its results.

The Sacred Harmonic Society, which has just entered the fortieth year of its existence, has, by its noble efforts in the cause of musical art, gained a most distinguished position. It is an institution of which this country has reason to be proud, for its reputation extends throughout the whole civilized world. It has some thousands of pounds invested in Government securities; it possesses a fine library, and there is, in immediate connection with it, a Benevolent Fund, out of which help is given to cases of distress. But its claims to our admiration are not yet exhausted, for to the Sacred Harmonic Society do we owe the establishment of the Handel Festivals, which have had a great educating influence upon the whole kingdom from so many choirs and choral societies taking part in them, and having thus had their standard of performance considerably raised. The Sacred Harmonic Society distributes annually many hundreds of pounds in the engagement of artistes, thus doing good service to those who, possessed of natural talent and genius, have had sufficient perseverance to qualify themselves for the higher positions in the world of art.

After the example of a town society, the proceedings of an

Amateur Society in the country shall be considered. The Herefordshire Philharmonic Society, established in 1863, consists of performing and non-performing members. There is an orchestra of about forty, eight or nine of whom are professional assistants, and a chorus of 120. Two concerts are annually given, and for each of these there are five rehearsals. Performing members are not allowed to take part in the performance unless they have attended at least three of these rehearsals, and when it is known that the distances to be travelled form a circle of which the diameter exceeds 100 miles, it will be understood that some zeal is displayed, and that some social good has been derived from the institution of the Society, otherwise the attendance of members would flag. The fact that a long list of names awaits election as vacancies occur is the best proof of the diligence of the members.

That the standard of the society is not a low one will be apparent from the programmes, each of which contains a symphony, a selection from an oratorio, or cantata, madrigals, part-songs, choruses, overtures, &c. All the solos and concerted pieces are sung by members, and there are four ladies who have respectively played, with orchestral accompaniments, Beethoven's Choral Fantasia, Mendelssohn's Andante and Rondo, Weber's Court-stück and Beethoven's Concerto in C minor; and at the next concert a gentleman is to play the first and second movements of Mendelssohn's Violin Concerto. The practical experience gained by the members in the preparation of classical works of this standard for public performance has enormously developed the musical capabilities of the district. Another important consideration is, that work such as the members do renders them far better qualified to enjoy and appreciate a purely professional performance when they have the opportunity of hearing one. Thus does the Herefordshire Philharmonic Society promote an extremely beneficial influence upon art, while the means of achievement are pleasantly social.

No doubt many who are present have heard of Welsh Eisteddfods, which are national gatherings in honour of the fine arts. The proceedings ordinarily consist of competitions by poets, solo singers, harpists, pianists, volunteer and other bands, and choral societies, but occasionally an oratorio or cantata is performed. Painting and sculpture are but seldom represented, but orators are highly appreciated.

Although things are done which, from their quaintness, are almost ludicrous, and the mistaken zeal of ardent supporters of the nationality causes the offering of prizes for subjects which raise a smile, still there is such unmistakable appreciation of all that is good, and such hearty enjoyment of it amongst the thousands who come from great distances to attend these Eisteddfods, that at once the thought comes uppermost, "What might not such a people do in art?" The soil exists, and all that is required is good cultivation.

If the Welsh only knew the musical strength they possess they would not fritter it away at these Eisteddfods as they do at present.

A strong permanent council and staff are wanted to supersede local jealousies, so that the whole national strength should be put forth. Just on the same principle that obtains when the Royal Agricultural Society of England visits any town for its annual meeting, so should the council take absolute command of the proceedings in order to carry out the Eisteddfod from a national point of view. If such a system as this could be adopted, the musical portion would, in a very few years, become quite as interesting, from an art point of view, as any of the smaller festivals of England. And this might easily be done without superseding aught that is deemed essential to an Eisteddfod by the Welsh.

The Chief Constable of Chester, in his Report for the year ending Sept. 29, 1870, thus speaks of the Saturday Evening Concerts:—"I may perhaps be allowed to repeat here, what I stated three years ago, namely, that with the opening of the Saturday Evening Entertainments a considerable decrease in the amount of drunkenness takes place. The temporary improvement is, I feel sure, due to no merely accidental coincidence, but rather directly owing to the fact, that large numbers of young men spend their Saturday evenings at the Music Hall, who otherwise would probably be found at the public-house."

The same result attended the establishment of Saturday Evening Concerts in Glasgow by a temperance association, called "The Glasgow Abstainers Union." The magistrates recognize these concerts as a most valuable auxiliary in keeping the streets quiet on a Saturday night, in the prevention of drunkenness and brawls, and in the improvement of that healthy moral tone which is the great desideratum of all who are in authority. Glasgow presents the most forcible example that can possibly be given of the social influence of music, for, previous to the establishment of the concerts, pulpit, platform, and press had vainly endeavoured to stem the tide of the drinking habits of the people, which were associated with all their ordinary amusements and enjoyments. Space does not allow the details, which are most interesting, of this most successful experiment.

In that wonderful colony, Saltaire, music is used as a recreation for the mill-workers. In the Penrhyn slate quarries, where I once heard the choruses of "The Messiah" sung by the workmen and their wives and children, music exercises its refining influence. In Florence the same movement has been started, and is working with excellent results: in fact, the difficulty lies, not in securing examples of the good effects of music, but in the enormous number which present themselves for illustration.

While these efforts have been made by the people themselves, the higher interests of art have been fostered by such institutions as the Festivals of Birmingham, Norwich, Hereford, Gloucester, Worcester, Bradford, and Leeds, the Philharmonic Society, the Italian Operas, the Sacred Harmonic Society, the Crystal Palace Concerts, the Handel Festivals, the National Choral Society, the New Philharmonic

Society, the Musical Union, the Oratorio Concerts, the Monday Popular Concerts, Mr. Henry Holmes's Musical Evenings, Mr. Halle's Concerts in Manchester and other northern cities, the Liverpool Philharmonic Society, and numberless choral and other societies of more or less fame. The mere fact that such associations exist speaks volumes for the heartiness of the people in their appreciation of music, and the great influence that art is exercising in almost every direction throughout the United Kingdom.

Municipal corporations and school boards should take advantage of such an influence, when, by the examples already quoted, it is proved how much may be done in the making of better citizens by the employment of such an innocent means of amusement as the cultivation of music presents.

With facts such as have been brought forward it can hardly be said that England is not a musical nation; but when it is said that English taste requires elevating, one feels that such an assertion is unfortunately only too true. This being the case, to whose fault must be laid the charge that English taste is not what it should be, considering the natural genius of the nation? Undoubtedly our Government is in fault, for it does next to nothing for an art which has the power of refining the people by its refining influence. What help Government does afford will presently appear.

In the grants made by Parliament for the year ending March 31st, 1872, the total amount voted was 233,179*l.* The following are items in the account:

	£
Schools of Science and Art	87,830
School of Mines and Geological Museum	10,403
College of Chemistry	680
Royal College of Science (Dublin)	6,913
Edinburgh Museum	8,824

In face of the above figures, it may well be asked why Music does not receive a like support? And why a system which works so well in other arts could not, with equally good results, be applied to music? Can it be that all our politicians are men who "have no music in their souls," and do not class Music amongst the Fine Arts? It is more than probable, considering the persistent neglect with which it is treated by them, that their only knowledge of it is as an accessory of pageants, an illustrator of toasts, or an accompaniment to dancing!

The principal demand of musicians is, that in the metropolis a National Institution should be established, which should be the heart of musical England; which should enable all who desire to follow music as a profession, to obtain a thorough theoretical and practical education; and which should have, as its governing body, such an array of our best men, that a musical degree conferred by it would be the greatest honour a musician could obtain.

With such a Central Institution, purely elementary musical edu-

cation might be left to the local school boards, and to such academies as might be formed by municipal authorities or by individuals.

Left to themselves and their own resources, the people of Great Britain are endeavouring to solve the problem of musical education, for, fully appreciating the benefits to be derived therefrom, musical education they are determined to have.

Mr. Hullah and Mr. Curwen have done great service to the cause by their steady endeavours to improve the present state of affairs, and have obtained satisfactory results. But official help is required, that the various isolated efforts which are being made should be concentrated into one great endeavour to improve the existing state of affairs. Our church services ought to be far better than they are, and the authorities of our cathedrals must be made sensible that apathy will never organize even decent performances. If music is to form an adjunct to the services, it ought to be the best of its kind.

Some persons imagine that music has an enervating influence, but where are the proofs? Do our schoolboys deteriorate because they have a knowledge of crotchets and quavers? are they less manly? or are they less gentlemen? And do their masters complain that music destroys their capacity for school work? An emphatic negation to such insinuations is given by experience; for the boys are, by a knowledge of music, brought more under the beneficial effects of female society and home influence, while the masters are anxious for still further musical development in their schools.

For more than half a century Germany has had elementary musical education in the national schools, while conservatories, more or less connected with the Government, have attended to the higher musical education of its people, and Germany is justly entitled to be considered the most musical nation in the world; but no one will venture the proposition that, in the late lamentable war, enervation was one of the national failings. On the contrary, intense vigour was the chief characteristic of the German tactics, and Song had much to do in knitting together their national bonds, for the poet and the musician who were the authors of the "Wacht am Rhein" rendered enormous service to their country by the enthusiasm roused whenever that poetic embodiment of national ideas was sung by the German troops.

We ourselves, the inhabitants of these happy isles, have also lately found that Song could do something for us, as the frequent performance of "God Save the Queen," and "God Bless the Prince of Wales," on every possible opportunity, has amply testified.

And now, a Central Institution being required, how is it to be formed? Without the slightest trouble. For one exists with complete organization, ready at a moment's notice to be utilized—and that institution is the Royal Academy of Music. Established in 1822, it has done much to elevate the standard of music in this country. It continued in flourishing circumstances until 1847, when it possessed 10,000*l.* in Government securities. From this prosperous condition

it gradually declined until 1868, when, in March, the committee of noblemen and gentlemen who managed the institution, and who had done their best, willingly giving up their time in its interests, resigned their functions, all the investments having been absorbed in the general expenditure, and a balance on the wrong side being left as a legacy to the future directors.

In this dire extremity, without a management and with a debt of 200*l.* weighing heavily upon them, the Professors of the Institution came to the rescue, Sir W. S. Bennett retaining the post of Principal, to which he had been appointed in 1866.

They cleared off the debt, and closed the proceedings of 1869 with a balance of 650*l.* in hand, and an increase of twenty-five pupils. But to do this, the Professors took only 45 per cent. of their usual fees, thus virtually making the Academy a present of the remaining 55 per cent.

In continuous prosperity have affairs progressed up to the present time, when the students number 144, and the balance in hand is some 2000*l.* There is no intention of hoarding this balance, for the greater part of it will be immediately applied to the restoration of those scholarships which had become extinct under the former committee, in consequence of the application of the funds set apart for them to the general requirements.

Comment is needless. An institution that has been restored by the energy and self-abnegation of its professors, when the working of it had failed in the hands of men of the highest social position, cannot but be deemed worthy of national support.

To a limited extent the Academy has received that support; for a grant of 500*l.* from Government was made to it in 1864, and was continued for 1865, 6, and 7. In 1868 it was withdrawn, but, thanks to Mr. Gladstone (and the earnest thanks of all musicians are due to him for that timely help), the grant was renewed in 1869, and has since then been continued.

The Academy, having its certificated pupils in every town of any consideration, might be of considerable use to school boards in supplying examiners where musical education has been established.

If Government would only assist the development of the Academy by help (which might be given on the principle of proportion to results) and active official recognition by the appointment of its representatives on the acting committee, a National Institution might be established which would be of immense service to art, a great stimulus to musical education, and which would elevate the taste of the great body of the people through the length and breadth of the kingdom.

[H. L.]

WEEKLY EVENING MEETING,

Friday, March 1, 1872.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

C. WILLIAM SIEMENS, D.C.L. F.R.S. M.R.I.

On Measuring Temperatures by Electricity.

THE truth revealed to us by one of the younger branches of physical science, which has been cultivated and expounded nowhere more effectually than within these walls, has divested heat and electricity of their mysterious character, and has taught us to regard them simply as "modes of motion."

Light also has been shown to be identical in its nature with heat, and the only remaining physical agency, "chemical affinity," has been recognized as a force differing only in "quality of action" from the others. According to these views, force, in whichever type of action it presents itself, is as *indestructible as matter itself*, and is therefore capable of being *stored* up and *measured* with the same certainty of result. We have a unit of force or the foot lb., and a unit of heat, or the heat necessary to raise the temperature of 1 lb. of water through one degree Fahr., and it has been already proved that 772 units of force are the equivalent value of one unit of heat. Again, the chemical force residing in 1 lb. of pure coal is equal to about 14,000 heat units, or $14{,}000 \times 772 = 10{,}808{,}000$ ft. lbs. = 4825 tons lifted one foot high.

Questions regarding the quantitative effects of heat present themselves, however, much less frequently for our consideration than questions regarding its *intensity*, upon which depends the nature of the phenomena surrounding us at every step, both in science and in ordinary life. The instrument at our command for determining moderate intensities or temperatures, the mercury thermometer, leaves little to be desired for ordinary use; but when we ascend in the scale of intensity, we soon approach a point when mercury boils, and from that point upward we are left without a reliable guide. The result is, that we find in scientific books on chemical processes, statements to the effect that such or such reaction takes place at a *dull red* heat, such another at a *bright red*, a *cherry red*, a *blood red*, or a *white* heat—expressions which remind one rather of the days of alchemy than of chemical science of the present day.

There are pyrometers, it is true, but these are either of a complex nature, or little reliance can be placed on them.

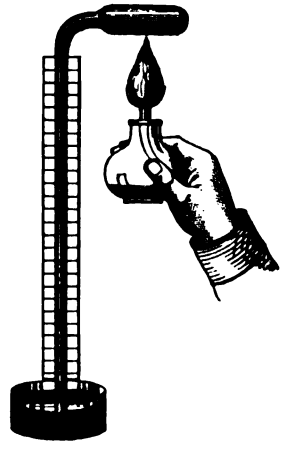
It is my purpose this evening to place before you an instrument

by which I hope to fill up to some extent the existing gap. It is the result of occasional experimental research, spread over several years, and it aims at the accomplishment of a double purpose, that of measuring high temperatures, and of measuring with accuracy the temperatures of *inaccessible* or *distant* places.

But before entering upon the details of my subject, I propose to place before you an instrument which fulfils, in principle, all the conditions essentially necessary in thermometry, and is at the same time the very first instrument that was ever proposed for measuring temperatures. I speak of the *air thermometer* by Galileo! It can be shown on theoretical grounds, that the expansion of a permanent gas at constant pressure is the most perfect index of temperature. It is in fact, the degree of energy of the atomical motion in an elastic fluid which determines its volume, and which constitutes at the same time its temperature.

The air thermometer consists simply of a bulb of glass with a long tubular stem, open to the atmosphere at its extremity. If I heat the bulb (by dipping it for instance into boiling water) and put it into a holder, with the hollow stem reaching downward into a cup of mercury, the air within the bulb will no longer communicate directly with the atmosphere, because the mercury is interposed. If now I cool the air within the bulb, by the external application of iced water its heat motion will diminish, and its volume would be reduced proportionally, if the external atmosphere could enter freely to fill up the vacancy thus created. But inasmuch as the external air cannot enter, a reduction of pressure will take place, which, according to the law of elasticity by Boyle, must be proportionate to the reduction of volume at constant pressure. The difference of pressure thus created between the bulb and the external atmosphere will be balanced by the column of mercury rising up into the tube, and the elevation to which the mercury attains is a true index of the temperature to which the air in the bulb had been previously heated. This is true with regard to all temperatures, from the lowest to the highest, and the instrument may be termed a universal thermometer. If the bulb could be cooled down to 273° Centigrade below the zero point, it would follow by the law of Charles that the elastic pressure of air would be reduced to nothing, that is to say, the motion of the particles of air, which we call heat, would have ceased, and we should

FIG. 1.



have reached the point of absolute zero, a point which has been theoretically established also by other means.

Practically, such an instrument would be most inconvenient; its indications would have to be corrected by calculation for barometrical variations; the capacity of the descending tube, which contains air not subjected to variation of temperature, would have to be taken into account, and no reliable observations could be arrived at, without taking special precautions, such as are only within reach of the experimental physicist.

[The other known methods of measuring ordinary and furnace temperatures were here passed in review, and the limits of their application pointed out. They were classified into:

Thermometers, by expansion of liquids.
Thermometers, by the expansion of solids.
Pyrometers, by chemical decomposition of solids, comprising Wedgwood's and Deville's pyrometers.
Pyrometers, by observing the melting-point of metals.
Pyrometers, by thermo-electricity.
Pyrometers, by exposing a copper or platinum ball of known heat capacity to the heat to be ascertained, and of quenching it in a measured quantity of water.]

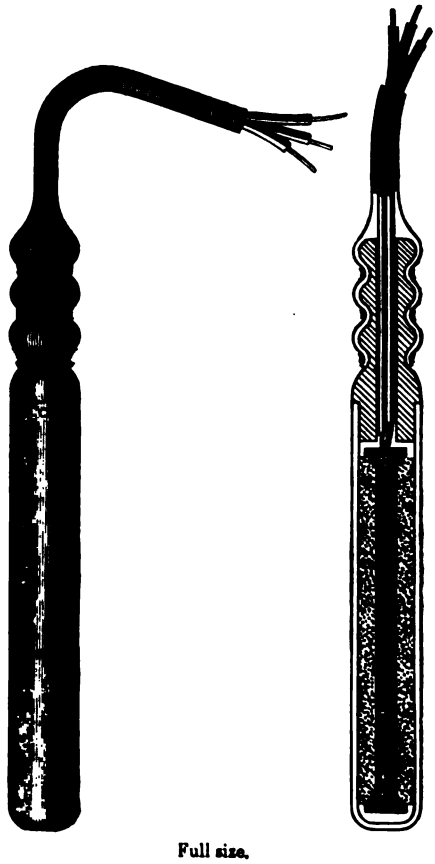
The instrument which forms the subject matter of my discourse presents many points of analogy with the air thermometer, if we substitute "*electrical resistance in conductors*" for "*expansion of gases*"! Both these effects are functions of temperature, increasing with the temperature according to progressive laws, which in the case of the gases we call the *law* of *Charles*, and in the case of conductors, the law of "*increase of electrical resistance with temperature.*" The latter law, which is of recent origin, had already been partially developed by Arndsen, Swanberg, Lenz, and Werner Siemens, when my attention was directed, in 1860, towards an application of the same to the measurement of temperatures at places inaccessible to the ordinary thermometer. By means of the contrivance which I shall describe presently, I was enabled to tell, in the testing cabin of a cable ship, the increasing temperature of the interior of the mass cable in the hold, and to prove the necessity of transhipment of the same into a vessel fitted with water-tight tanks, which have been resorted to ever since, to avoid the danger of softening the gutta-percha covering.

I have arranged an apparatus for proving to you in the first instance that the conductivity of a wire of platinum or other metal is greatly influenced by its temperature; for this purpose I direct the current of a galvanic battery at will through two branches of equal resistance, each branch comprising a free spiral wire of platinum and one of the coils of a differential galvanometer. By throwing the powerful light of an electric lamp upon the face of the differential galvanometer, and by throwing the image by means of a mirror and lens upon the screen, the audience will see any movement of the needle to the right or the left that may take place when I complete the battery connection. The

resistance of both branch circuits being the same, no deflection of the needle is observable on depressing the key, but when I pass the flame of a spirit-lamp under the one platinum coil the needle is thrown immediately over to the right, because the electrical resistance of the heated wire is increased, and consequently a larger proportion of the current is passing through the cooler circuit, exercising a preponderating influence upon the galvanic needle. When I withdraw the spirit flame from the wire the needle rapidly returns to its zero position, but in passing it under the other spiral wire the needle immediately deflects in the opposite direction.

If instead of using the open spirals I were to wind thin insulated wire of any pure metal upon two small cylindrical pieces of wood, and were to enclose the tiny spirals in small silver casings, as shown in view and in section by Fig. 2, taking care that the extremities of the spiral wires were soldered to thicker insulated wires leading respectively to the battery and differential galvanometer before mentioned, it follows that no deflection of the needle ensues when both the protected and equal spirals are dropped into a jar containing iced water. But if I take one of the spirals from the water, and place it, for instance, by his kind permission, into the hand of our President without disconnecting the same from its leading wires, the balance of resistance will no longer take place, and a deflection of the needle to the right actually takes place.

Fig. 2.



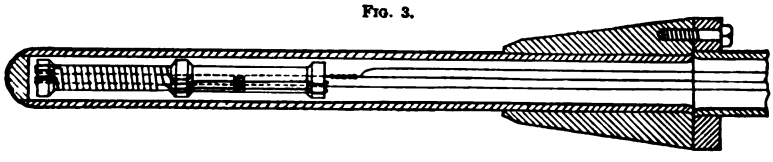
Full size.

I will now endeavour, however, to re-establish the equilibrium by adding warm water to the iced water surrounding the comparison coil near me until no deflection of the needle is observable. This result being obtained, it follows that the temperature of the water

surrounding the one coil must be identical with the temperature of our President's hand, and the delicate mercury thermometer which I have placed in my solution must give me the temperature of the distant place which I intended to measure. The temperature here observed is 89·5° Fahrenheit, which is at this moment that of Sir Henry Holland's hands. This result is independent of the ratio in which the electrical resistance increases with temperature in the similar coils, and considering that the silver casings containing the coils are not larger than small pencil-cases, this method might be advantageously employed in physiological research. The one coil would only have to be placed within the cavity to be measured to enable the observer to read the temperature from time to time, without disturbing the patient, with the accuracy of which the mercury or spirit of wine thermometer employed is capable. But the same method is applicable for measuring the temperatures of distant or inaccessible places, such as the interior of stores or cargoes of materials liable to spontaneous combustion; of points elevated above the surface of the ground; or of great depths below for meteorological purposes; or for measuring the temperature of the sea continuously in attaching such a coil to the mariner's sounding lead. An error would in such cases arise, however, through the uncertainty of the resistance of long leading wires, if a complete remedy of error from such a source had not suggested itself. This consists in uniting three separate insulated leading wires, into a cable by which the distant coil is connected with the measuring instrument. One galvanic circuit passes from the battery through one of the leading wires, through the distant spiral and back again through the second leading wire to the differential galvanometer and the battery, and the second passes from the same battery through the near coil, and through the third leading wire up to the distant coil without traversing the same, and back again through the second leading wire to the galvanometer and battery. Thus both galvanic circuits comprise the leading wires up to the distant coil, and all variations of resistance by temperature to which the leading wires may be subjected, affect both sides of the balance equally. In constructing coils for measuring deep-sea temperatures a large quantity of insulated copper or iron wire is wound upon a metallic tube open at both ends to admit the sea-water freely in order to impart its temperature to the innermost layers of the insulated wire. The coil of wire is protected externally by drawing a tube of vulcanized india-rubber over it, which in its turn is bound round by a close spiral layer of copper wire, whereby the sea-water is effectually excluded from the sensitive coil. By these arrangements the temperature of distant or otherwise inaccessible places can be accurately ascertained; but the method is limited to the range of temperature which can be obtained and measured in the comparison bath. In order to realize a pyrometer by electrical resistance, it is necessary to rely upon the absolute measurement of the electrical resistance of a coil of wire which must be made to resist intense heats without deteriorating

through fusion or oxidation. Platinum is the only suitable metal for such an application, but even platinum wire deteriorates if exposed to the direct action of the flame of a furnace, and requires an external protection. The platinum wire used has, moreover, to be insulated and supported by a material which is not fused or rendered conductive at intense heats, and the disturbing influence of leading wires had in this case also to be neutralized. These various conditions are very fully realized by the arrangement represented on the following diagram, Fig. 3.

Fig. 3.



Thin platinum wire is coiled upon a cylinder of hard-baked porcelain, upon the surface of which a double-threaded helical groove is formed for its reception, so as to prevent contact between the coils of wire. The porcelain cylinder is pierced twice longitudinally for the passage of two thick platinum leading wires, which are connected to the thin spiral wire at the end. In the upper portion of the porcelain cylinder the two spiral wires are formed into a longitudinal loop, and are connected crossways by means of a platinum binding screw, which admits of being moved up or down for the purpose of adjustment of the electrical resistance at the zero of Centigrade scale. The porcelain cylinder is provided with projecting rims, which separate the spiral wire from the surrounding protecting tube of platinum, which is joined to a longer tube of wrought iron, serving the purpose of a handle for moving the instrument. If the temperatures to be measured do not exceed a moderate white heat, or say 1300° Centigrade = 2372° Fahr., it suffices to make the lower protecting tube also of wrought iron, to save expense. This lower portion only, up to the conical enlargement or boss of iron, is exposed to the heat to be measured. Three leading wires of insulated copper united into a light cable connect the pyrometer with the measuring instrument, which may be at a distance of some hundred yards from the same. They are connected by means of binding screws at the end of the tube to three thick platinum wires passing down the tube to the spiral of thin platinum wire. Here two of the leading wires are united, whereas the third traverses the spiral, and joins itself likewise to one of the two former, which forms the return wire for two electrical circuits, the one comprising the spiral of thin wire, and the other returning immediately in front of the same, but traversing in its stead a comparison coil of constant resistance. The measuring instrument may consist of a differential galvanometer as before, if to the constant resistance a variable resistance is added. If the pyrometer coil were to be put

into a vessel containing snow and water, the balance of resistance between the two battery circuits would be obtained without adding variable resistance to the coil of constant resistance, and the needle of the differential galvanometer would remain at zero when the current is established. But on exposing the pyrometer to an elevated temperature the resistance of its platinum coil would be increased, and resistance to the same amount would have to be added to the constant resistance of the measuring instrument, in order to re-establish the electrical balance. This additional resistance would be the measure of the increase of temperature, if only the ratio in which platinum wire increases in electrical resistance with temperature is once for all established. This is a question which I shall revert to after having completed the description of the pyrometric instrument.

Although I have explained that by means of a differential galvanometer and a variable resistance (constituting in effect a Wheatstone bridge arrangement) the increasing resistance of the platinum spiral may be measured, it was found that the use of a delicate galvanometer is attended with considerable practical difficulty in iron-works and other rough places where it is important to measure elevated temperatures, or on board ship for measuring deep-sea temperatures. I was therefore induced to seek the same result by the conception of an instrument which is independent in its action from tremulous motion, or from magnetic disturbance caused by moving masses of iron, and which requires no careful adjustment or special skill on the part of the operator. This instrument is represented by Fig. 4 on page 446, and may be termed a chemical resistance measurer or "differential voltameter." The immortal Faraday has proved that the decomposition of water in a voltameter expressed by the volumes of gases V, is proportionate in the unit of time to the intensity I of the decomposing current, or that

$$I = \frac{V}{T}.$$

According to Ohm's general law, the intensity I is governed by the electro-motive force E, and inversely by the resistance R, or it is

$$I = \frac{E}{R}.$$

It is therefore

$$\frac{V}{T} = \frac{E}{R} \text{ or } V = \frac{E\,T}{R};$$

or the volume V would give a correct measure of the electrical resistance R if only the electro-motive force E and time T were known and constant quantities. But the electro-motive force of a battery is very variable; it is influenced by polarization of the electrodes, by temperature, and the strength and purity of the acid employed. The volume of gases obtained is influenced, moreover, by the atmospheric

pressure, and it is extremely difficult to make time observations correctly. It occurred to me, however, that these uncertain elements might be entirely eliminated in combining two similar voltameters in such a manner that the current of the same battery was divided between the two, the one branch comprising the unknown resistance to be measured, and the other a known and constant resistance. The volume of gas V^{I} produced in this second voltameter, having a resistance R^{I} in circuit, would be expressed by

$$V^{I} = \frac{E\,T}{R^{I}},$$

and we should have the proportion of

$$V : V^{I} = \frac{E\,T}{R} : \frac{E\,T}{R^{I}};$$

or E and T, being the same in both cases, may be struck out, and the expression will assume the simple form

$$V : V^{I} = R^{I} \div R.$$

The constant resistance R of the one circuit being known, it follows that the unknown resistance R^{I} is expressed by $\frac{R^{V}}{V^{I}}$; that is to say, by a constant multiplied by the proportion of gas produced in the two voltameters irrespective of time, or strength of battery, or temperature, or the state of the barometer.

The resistance R and R^{I} are composed each of two resistances, namely, that of the principal coils, which we may term R or R^{I}, and of the voltameter and leading wires, which is the same in both cases, and may be expressed by y. The expression should therefore be written as follows:

$$V : V^{I} = R^{I} + y^{I} : R + y,$$

R^{I} being the unknown quantity.

The mechanical arrangement of the instrument will be understood from the diagram, Fig. 4; and the whole arrangement of the pyrometer, with its leading wire and resistance measurer, from the general view given in Fig. 5. The voltametric resistance measurer consists of two calibrated vertical tubes of glass of about 3 millimètres diameter, which are fixed upon a scale showing arbitrary but equal divisions. The upper ends of the tubes are closed by small cushions of india-rubber pressed down upon the openings by means of weighted levers, whereas the lower portions of the tubes are widened out and closed by plugs of wood, through which the electrodes in the form of pointed platinum wires penetrate to the depth of about 25 millimètres into the widened portions of the tubes. By a side branch the widened portion of each vertical tube communicates by means of an india-rubber connecting pipe to a little glass reservoir containing acidulated water, and supported in a vertical slide. In raising the weighted cushions closing

the upper ends of the vertical tubes, and in adjusting the position of the small reservoirs, the acidulated water will rise in both tubes to the zero line of the scale. In turning a button in front of the tubes the battery current is passed through both pairs of electrodes, the one circuit comprising the permanent resistance R and the leading wires up to the pyrometer, and the other the leading wires and the pyrometer coil. If the resistance of the pyrometer coil should be equal to the permanent resistance R, the $R' + y$ will be equal to $R + y$, and therefore $V = V'$, but as the resistances differ, so will the volumes. Necessary conditions are: that both reservoirs are filled with the same standard solution of pure water with about 10 per cent. of sulphuric acid, that all the electrodes are of the same form and size, and that their polarity is reversed frequently during the progress

Fig. 4.

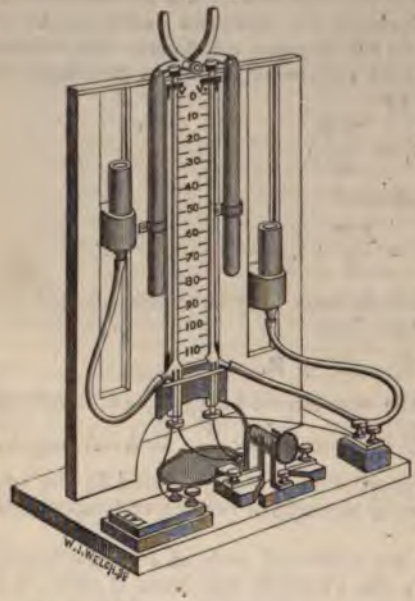
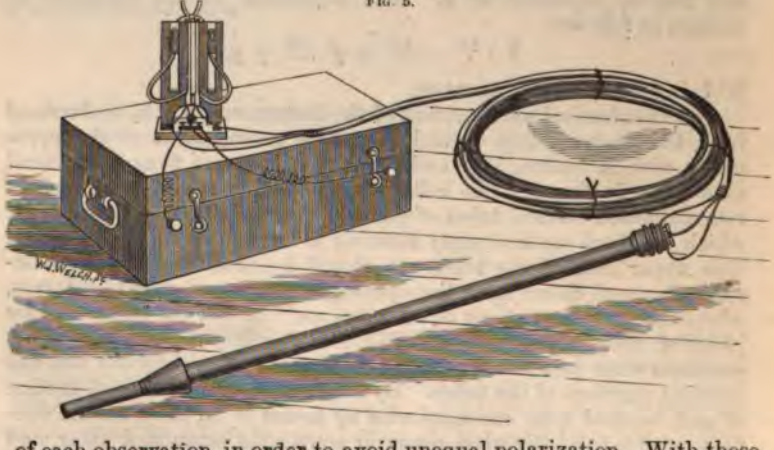


Fig. 5.



of each observation, in order to avoid unequal polarization. With these precautions, which involve no particular skill or knowledge of elec-

trical observation on the part of the operator, very accurate results are obtained; but in order not to incur considerable error of observation it is advisable to continue the current, reversing the same say twice, until at least forty divisions of gases are produced in the least activated tube, which operation will occupy from 2 to 3 minutes; if a battery, of from 4 to 6 Daniell elements is employed. The volumes V and V^1 being noted, after having allowed half a minute for the gases to collect after the current has ceased, the weighted cushions upon the tubes are raised in order to allow the gases to escape, when the water levels will immediately return to their zero position, to make ready for another observation. By inserting the observed values for V and V^1 into the expression above given, the unknown resistance R^1 can be easily calculated; but in order to facilitate the use of the instrument I have prepared a Table which gives at a glance the resistance due to any two observed volumes, the volumes V governing the vertical, V^1 the horizontal columns, and the resistance being read off at the point of intersection. At each point of intersection the resistance is marked in black, and the corresponding temperature in red ink.

It now remains only to be shown what is the relation between the resistance and temperature in heating a platinum wire. The researches of Dr. Matthiesen, who has made the latest investigations on the effect of temperature upon electrical resistance, are restricted to the narrow range of temperatures between 0° and 100° Centigrade, nor do they comprise platinum. He adopted the following general expression for the pure metals:

$$R_t = \frac{R_0}{1 + x\,t + y\,t^2},$$

which, in determining the specific values of x and y for each metal, gives a close agreement with observation between the narrow limits indicated, but is wholly inapplicable for temperatures exceeding 200° Centigrade, when the value t^2 commences to predominate and to produce absurd values for R_t.

It was necessary for my purpose to undertake a series of elaborate experiments with a view of finding a ratio of general application. Coils of thin wire, of platinum, iron, copper, and some other metals, were gradually heated and cooled in metallic chambers containing the bulbs of mercury thermometers, and for higher temperatures of air thermometers, and the electrical resistances were carefully noted. The progressive increase of electrical resistance was thus compared directly with the increasing volume of a permanent gas (carefully dried) between the limits of zero and 470° Centigrade and a ratio established, which is represented by the formula

$$R_t = \alpha T^2 + \beta T + \gamma,$$

in which T signifies total temperature counting from the absolute zero, and α β and γ specific coefficients for each metal. According to this formula the electrical resistance is a constant at the absolute zero, and

progresses in a ratio represented graphically by a tipped-up parabola, approaching more and more toward a uniform ratio at elevated temperatures. Although the comparison with the air thermometer could only be carried up to 470° Centigrade, the general correctness of the ratio of increase just stated has been verified by indirect means in measuring progressive heats, and by comparison with the platinum ball pyrometer.

It is important to mention here that great care must be exercised in the selection of the platinum wire for the measuring spiral, platinum wire having been met with conducting only 4·7 times better than mercury at zero, Centigrade, and others; conducting 8·2 times better than mercury, although both samples had been supplied by the same eminent makers, Messrs. Johnson and Mathey. The abnormal electrical resistance of some platinum wire is due chiefly to the admixture of iridium or other metals of the same group, and it appears that the platinum prepared by the old welding process is purer and therefore better suited for electrical purposes than the metal consolidated by fusion in a Deville furnace.

In conclusion, I shall show some working results of the pyrometer in measuring by means of the same protected coil a mixture of ice and water, boiling water, molten lead, and the fire itself by which the lead is melted, the readings produced being 2° Centigrade, 98° Centigrade, 330° Centigrade, and 860° Centigrade respectively. The latter temperature signified a cherry red heat, as may be judged by the appearance of the tube when withdrawn from the fire. The instrument which I have had the honour to bring before you this evening has already received several useful applications. Through its first application an important telegraph cable was saved from destruction through spontaneous generation of heat. Prof. Bolzani, of Kasan, has made some interesting applications of it for recording the temperature at elevated points and at points below the earth's surface. Mr. Lowthian Bell has used it in his well-known researches on blast-furnace economy; and at several iron-works pyrometer tubes are introduced into the heating stoves, and permanently connected with the office, where the heat of each stove can at all times be read off and recorded. These and other applications are sufficiently self-evident, if the soundness of the principles upon which I rely is conceded; but I feel that the shortness of time at my command has hardly enabled me to do more than to pass these in review, while endeavouring to demonstrate the results obtained of recording the temperatures of distant or inaccessible places, including furnace temperatures.

[C. W. S.]

GENERAL MONTHLY MEETING,

Monday, March 4, 1872.

Sir Frederick Pollock, Bart. M.A. Vice-President, in the Chair.

Sir Reginald Barnewall, Bart.
Francis Darwin, Esq. B.A.
Thomas Andros De la Rue, Esq. B.A.
Francis Dutton, Esq. F.R.G.S.
Charles Elam, M.D. F.R.C P.
J. Kennedy Esdaile, Esq. M.A.
Charles Few, Esq.
Hugh Ferguson, Esq.
Sir Donald Friell M'Leod, C.B. K.C.S.I.
Wm. Belger Gibbs, Esq. F.R.A.S.
Henry Grissell, Esq.
Albert Hovenden, Esq.
Alfred Bray Kempe, Esq. B.A.
Henry Robert Kempe, Esq.
Falconer Lockworthy, Esq.
George Lockyer, jun. Esq.
Mrs. E. H. Moscrop.
Sir Thomas Pycroft, K.C.S.I.
Charles Harley Savory, Esq.
Mrs. Basil Woodd Smith.
Charles Edward Seth Smith, Esq.

were *elected* Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Actuaries, Institute of—Journal, No. 86. 8vo. 1872.
British Architects, Royal Institute of—Sessional Papers, 1871-2. Nos. 4, 5. 4to.
Chemical Society—Journal for Jan. 1872. 8vo.
Dublin Society, Royal—Journal, No. 40. 8vo. 1872.
Edinburgh Royal Observatory—Scottish Meteorology, 1856-71. (Edin. Astron. Obs.) Vol. XIII. 4to. 1872.
Editors—Academy for Feb. 1872. 4to.
American Journal of Science for Jan. 1872. 8vo.
Artizan for Feb. 1872. 4to.
Athenæum for Feb. 1872. 4to.
Chemical News for Feb. 1872. 4to.
Engineer for Feb. 1872. fol.
Horological Journal for Feb. 1872. 8vo.
Journal of Gas-Lighting for Feb. 1872. 4to.
Mechanics' Magazine for Feb. 1872. 8vo.
Nature for Feb. 1872. 4to.
Pharmaceutical Journal for Feb. 1872. 8vo.
Photographic News for Feb. 1872. 4to.
Geological Institute, Imperial, Vienna—Jahrbuch, 1871. No. 3. 8vo.
Verhandlungen, 1871. Nos. 14, 15. 8vo.
London University—Calendar for 1872. 16to.
Manchester Literary and Philosophical Society—Proceedings. Vol. XI. Nos. 9, 10. 8vo. 1871-2.
Mann, W. Wilberforce, Esq. (the Author)—The Linn-base Decimal System. (O 14) 16to. 1872.
Mechanical Engineers' Institution, Birmingham—Proceedings, Oct. 1871. 8vo.
Meteorological Office—Quarterly Weather Report, 1870, Part 3. 4to. 1872.

Photographic Society—Journal, No. 230. 8vo. 1872.
Preussische Akademie der Wissenschaften—Monatsberichte. Nov. 1871. 8vo.
Royal Society of London—Proceedings, No. 130. 8vo. 1872.
Wedgwood, Hensleigh, Esq. M.R.I. (the Author)—Origin of Language. 8vo. 1872.
Würzburg Physical and Medical Society—Verhandlungen, Neue Folge. Band I. Band II. 1, 2. 8vo. 1868–71.

WEEKLY EVENING MEETING,

Friday, March 8, 1872.

WILLIAM SPOTTISWOODE, Esq. M.A. LL.D. Treas. R.S. and R.I. Vice-President, in the Chair.

R. LIEBREICH, Esq. M.R.I.
OPHTHALMIC SURGEON AND LECTURER AT ST. THOMAS'S HOSPITAL,

Turner and Mulready—On the Effect of certain Faults of Vision on Painting, with especial Reference to their Works.

WHEN I arrived in England about eighteen months ago, little thinking that a short vacation tour would end in my permanent residence here, I at once paid a visit to the National Gallery. I was anxious to see Turner's pictures, which on the Continent I had had no opportunity of doing. How great was my astonishment when, after having admired his earlier works, I entered another room which contained his later paintings! Are these really by the same hand? I asked myself on first inspecting them; or have they suffered in any way? On examining them, however, more closely, a question presented itself to my mind which was to me a subject of interesting diagnosis. Was the great change which made the painter of "Crossing the Brook" afterwards produce such pictures as "Shade and Darkness," caused by an ocular or cerebral disturbance? Researches into the life of Turner could not afford an answer to this question. All that I could learn was, that during the last five years of his life his power of vision as well as his intellect had suffered. In no way, however, did this account for the changes which began to manifest themselves about fifteen years before that time. The question could therefore only be answered by a direct study of his pictures from a purely scientific, and not at all from an æsthetic or artistic point of view.

I chose for this purpose pictures belonging to the middle of the period which I consider pathological, *i. e.* not quite healthy, and analyzed them in all their details, with regard to colour, drawing, and distribution of light and shade.

It was particularly important to ascertain if the anomaly of the whole picture could be deduced from a regularly recurring fault in its details. This fault is a vertical streakiness, which is caused by every illuminated point having been changed into a vertical line. The elongation is, generally speaking, in exact proportion to the brightness

of the light; that is to say, the more intense the light which diffuses itself from the illuminated point in nature, the longer becomes the line which represents it on the picture. Thus, for instance, there proceeds from the sun in the centre of a picture a vertical yellow streak, dividing it into two entirely distinct halves, which are not connected by any horizontal line. In Turner's earlier pictures, the disk of the sun is clearly defined, the light equally radiating to all parts; and even where through the reflexion of water a vertical streak is produced, there appears, distinctly marked through the vertical streak of light, the line of the horizon, the demarcation of the land in the foreground, and the outline of the waves in a horizontal direction. In the pictures, however, of which I am now speaking, the tracing of any detail is perfectly effaced when it falls in the vertical streak of light. Even less illuminated objects, like houses or figures, form considerably elongated streaks of light. In this manner, therefore, houses that stand near the water, or people in a boat, blend so entirely with the reflexion in the water that the horizontal line of demarcation between house and water or boat and water entirely disappears, and all becomes a conglomeration of vertical lines. Everything that is abnormal in the shape of objects, in the drawing, and even in the colouring of the pictures of this period, can be explained by this vertical diffusion of light.

How and at what time did this anomaly develop itself?

Till the year 1830 all is normal. In 1831 a change in the colouring becomes for the first time perceptible, which gives to the works of Turner a peculiar character not found in any other master. Optically this is caused by an increased intensity of the diffused light proceeding from the most illuminated parts of the landscape. This light forms a haze of a bluish colour which contrasts too much with the surrounding portion in shadow. From the year 1833 this diffusion of light becomes more and more vertical. It gradually increases during the following years. At first it can only be perceived by a careful examination of the picture, but from the year 1839 the regular vertical streaks become apparent to everyone. This increases subsequently to such a degree, that when the pictures are closely examined they appear as if they had been wilfully destroyed by vertical strokes of the brush before they were dry, and it is only from a considerable distance that the object and the meaning of the picture can be comprehended. During the last years of Turner's life this peculiarity became so extreme that his pictures can hardly be understood at all.

It is a generally-received opinion that Turner adopted a peculiar manner, that he exaggerated it more and more, and that his last works are the result of a deranged intellect. I am convinced of the incorrectness, I might almost say of the injustice, of this opinion. The word "manner" has a very vague meaning. In general we understand by it something which has been arbitrarily assumed by the artist. It may be the result of study, of reflection, of a development of principle, or the consequence of a chance observation, of an experi-

ment, or of an occasional success. Nothing of all this applies to what has been called Turner's manner. Nothing in him is arbitrary, assumed, or of set purpose. According to my opinion, his manner is exclusively the result of a change in his *eyes*, which developed itself during the last twenty years of his life. In consequence of it the aspect of nature gradually changed for him, while he continued in an unconscious, I might almost say in a *naïve* manner, to reproduce what he saw. And he reproduced it so faithfully and accurately, that he enables us distinctly to recognize the nature of the disease of his eyes, to follow its development step by step, and to prove by an optical contrivance the correctness of our diagnosis. By the aid of this contrivance we can see nature under the same aspect as he saw and represented it. With the same we can also, as I shall prove to you by an experiment, give to Turner's early pictures the appearance of those of the later period.

After he had reached the age of fifty-five, the crystalline lenses of Turner's eyes became rather dim, and dispersed the light more strongly, and in consequence threw a bluish mist over illuminated objects. This is a pathological increase of an optical effect, the existence of which, even in the normal eye, can be proved by the following experiment. If you look at a picture which hangs between two windows, you will not be able to see it distinctly, as it will be, so to speak, veiled by a greyish haze. But if you hold your hands before your eyes so as to shade them from the light of the windows, the veiling mist disappears, and the picture becomes clearly visible. The disturbing light had been diffused by the refracting media of the eye, and had fallen on the same part of the retina on which the picture was formed. If we examine the eye by an illumination resembling that by means of which Professor Tyndall, in his brilliant experiments, demonstrated to you the imperfect transparency of water, we find that even the clearest and most beautiful eye is not so perfectly transparent as we would suppose. The older we get the more the transparency decreases, especially of the lens. But to produce an effect equal to that visible in Turner's pictures after the year 1831, pathological conditions are required. In the years that followed, as often happens in such cases, a clearly-defined opacity was formed in the slight and diffuse dimness of the crystalline lens. In consequence of this the light was no longer evenly diffused in all directions, but principally dispersed in a vertical direction. At this period the alteration offers, in the case of a painter, the peculiarity that it only affects the appearance of natural objects, where the light is strong enough to produce this disturbing effect, whilst the light of his painting is too feeble to do so: therefore, the aspect of nature is altered, that of his picture correct. Only within the last years of Turner's life, the dimness had increased so much, that it prevented him from seeing even his pictures correctly. This sufficiently accounts for the strange appearance of his last pictures, without its being necessary to take into account the state of his mind.

It may seem hazardous to designate a period as diseased, the beginning of which art-critics and connoisseurs have considered as his climax. I do not think that the two opinions are in decided contradiction to each other. To be physiologically normal is not at all a fundamental condition in art; and we cannot deny the legitimacy of the taste which regards that which is entirely sound and healthy as commonplace, trivial, and uninteresting, and which, on the contrary, is fascinated by that which approaches the border of disease and even goes beyond it.

Many of the best musicians, for instance, and some of the greatest admirers of Beethoven, prefer his latest works, and consider them the most interesting, although the influence of his deafness upon them is apparent to others.

In poetry, we rank some poems among the highest productions of art in which the imagination of the poet goes far beyond the normal region of the mind:

> "The poet's eye, in a fine frenzy rolling,
> Doth glance from heaven to earth, from earth to heaven."

Thus it seems to me perfectly natural that the peculiar poetical haze which is produced by the diffusion of light in Turner's pictures after 1831 should have a particular attraction for many of Turner's admirers. On the other hand, passing over the faults, we discover in these pictures peculiar merits, and we recognize that the great artist continued in many ways to improve, even at a time of his life when his failing sight began to deprive his works of general favour. I cannot, however, defend the opinion of those who are enraptured with Turner's pictures belonging to a still later period—who consider a picture beautiful which, in consequence of this optical defect, is entirely disfigured and defaced, and who, calling this Turner's style, would like to form it into a school and imitate it. They resemble the porter of a certain dealer in works of art, who one day, when he had to deliver the torso of a Venus at a gentleman's house, answered the servant, who had expressed his astonishment that his master should have bought a thing without head, arms, or legs, "You don't understand; that's just the beauty of it."

I show you here first a picture which is copied from an oil-painting in the South Kensington Museum. This picture was not exhibited till the year 1833, but it was painted some time before, and from sketches taken in Venice previous to any change in Turner's sight. I shall now try so to change this picture, by an optical contrivance, as to make it resemble the pictures he painted after 1839. You must, of course, not expect to see in this rough representation, which a large theatre necessitates, anything of the real beauty of Turner's pictures. Our object is to analyze their faults.

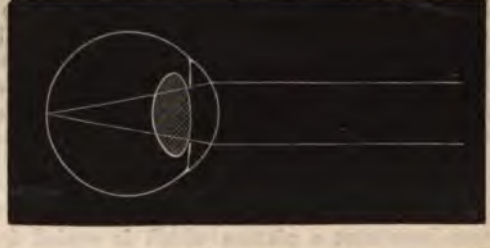
In order to show you in a single object what you have already observed in the general aspect of a picture, I choose purposely a tree, because there are no trees in the "Venice" you have just seen, and

more particularly because after the year 1833 Turner painted trees that were unknown to any botanist, had never been seen in nature, nor been painted by any other artist. I do not think it likely that Turner invented a tree he had never seen; it seems to me more probable that he painted such trees because he saw them so in nature. I searched for them with the aid of the lens, and soon discovered them. Here is a common tree; the glass changes it into a Turner tree.

Let us now turn from the individual case of a great artist to a whole category of cases, in which the works of painters are modified by anomalies in their vision—I mean cases of irregularities in the refraction of the eye. The optical apparatus of the eye forms, like the apparatus of a photographer, inverted images. In order to be seen distinctly these images must fall exactly upon the retina. The capacity of the eye to accommodate itself to different consecutive distances, so as to receive on the retina distinct images of objects, is called accommodation. This faculty depends upon the power of the crystalline lens to change its form. The accommodation is at its greatest tension if we adapt our eye to the nearest point. It is, on the contrary, in complete repose if we adapt it to the farthest point. The optical state of the eye during its adaptation for the farthest point, when every effort of accommodation is completely suspended, is called its refraction.

There are three different kinds of refraction: firstly, that of the normal eye; secondly, of the short-sighted eye; thirdly, of the over-sighted eye.

Fig. 1.

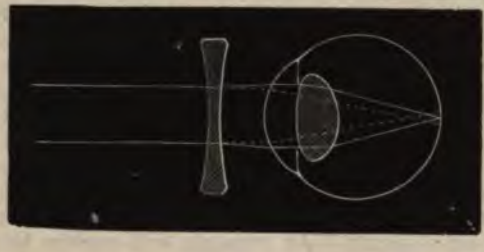


1. The normal eye, when the activity of its accommodation is perfectly suspended, is adjusted for the infinite distance; that is to say, it unites upon the retina parallel rays of light (Fig. 1).

2. The short-sighted eye has, in consequence of an extension of its axis, a stronger refraction, and unites therefore in front of the retina the rays of light which proceed from infinite distance. In order to be united upon the retina itself the rays of light must be divergent; that is to say, they must come from a nearer point. The more short-sighted the eye, the stronger must be the divergence; such an eye, in order to see distinctly distant objects, must make the rays from a distant object more divergent, by aid of a concave glass.

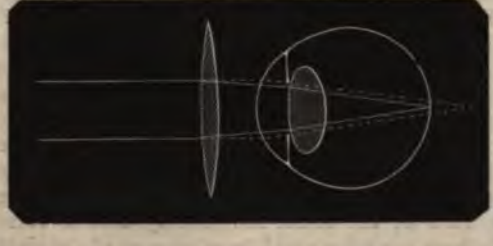
We determine the degree of short-sightedness by the power of the weakest concave glass that enables the eye to see distinctly at a great distance (Fig. 2).

Fig. 2.



3. The over-sighted, or hypermetropic eye, on the contrary, has too weak a refraction: it unites convergent rays of light upon the retina; parallel or divergent rays of light it unites behind the retina, unless an effort of accommodation is made. The degree of hypermetropy, or over-sightedness, is determined by the focal distance of the strongest convex glass with which objects can still be distinctly seen at a great distance (Fig. 3).

Fig. 3.



Hypermetropy has no essential influence upon painting; it only reduces the power of application, and must therefore be corrected by wearing convex glasses. This can never be avoided if the hypermetropy is so great as to diminish the distinctness of vision. Short-sightedness, on the contrary, generally influences the choice of the subject of the artist and also the manner of its execution. As a very small handwriting is an indication of short-sightedness, so we find that artists who paint small pictures, and finish the details with great minuteness and with fine touches of the brush, are mostly short-sighted.

Sometimes the shape of the eye diverges from its normal spherical form, and this is called astigmatism. This has only been closely investigated since Airy discovered it in his own eye. Figure to yourself meridians drawn on the eye as on a globe, so that one pole is placed

in front: then you can define astigmatism as a difference in the curvature of two meridians, which may, for instance, stand perpendicularly upon each other; the consequence of which is a difference in the power of refraction of the eye in the direction of the two meridians. An eye may, for instance, have a normal refraction in its horizontal meridian, and be short-sighted in its vertical meridian. Small differences of this kind are found in almost every eye, but are not perceived. Higher degrees of astigmatism, which decidedly disturb vision, are, however, not uncommon, and are therefore also found among painters. I have had occasion to examine the eyes of several distinguished artists which presented such an anomaly, and it interested me much to discover what influence this defect had upon their works. The diversity depends in part upon the degree and nature of the optical anomaly, but its effect shows itself in different ways, according to the subjects the artist paints. An example will explain this better. I know a landscape-painter and a portrait-painter who have both the same kind of astigmatism; that is, the refraction of the vertical meridian differs from the refraction of the horizontal one. The consequence is, that their sight is normal for vertical lines, but for horizontal lines they are slightly short-sighted. Upon the landscape-painter this has hardly any disturbing influence. In painting distant views sharp outlines are not requisite, but rather undefined and blending tones of colour. His eye is sufficiently normal to see these. I was struck, however, by the fact that the foreground of his pictures, which generally represents water with gently-moving waves, was not painted with the same truthfulness to nature as the middle and back ground. There I found short horizontal strokes of the brush in different colours, which did not seem to belong to the water. I therefore examined the picture with a glass, which, when added to my eye, produced the same degree of astigmatism as existed in the painter's eye, and the whole picture appeared much more beautiful, the foreground being now as perfect as the middle and back ground. In consequence of this artificially-produced astigmatism, I saw the horizontal strokes of the brush indistinctly, and so mixed together, that through them the colour and transparency of the water were most exquisitely rendered.

Upon the portrait-painter astigmatism had a very different influence. He was held in high esteem in Paris, on account of his excellent grasp of character and intellectual individuality. His admirers considered even the material resemblance of his portraits as perfect; most people, however, thought he had intentionally neglected the material likeness by rendering in an indistinct and vague manner the details of the features and the forms. A careful analysis of the picture shows that this indistinctness was not at all intentional, but simply the consequence of astigmatism. Within the last few years the portraits of this painter have become considerably worse, because the former indistinctness has grown into positively false proportions. The neck and oval of the face appear in all his portraits considerably

elongated, and all details are in the same manner distorted. What is the cause of this? Has the degree of his astigmatism increased? No; this does not often happen: but the effect of astigmatism has doubled, and this has happened in the following manner:—An eye which is normal as regards the vision of vertical lines, but short-sighted for horizontal lines, sees the objects elongated in a vertical direction. When the time of life arrives that the normal eye becomes far-sighted, but not yet the short-sighted eye, this astigmatic eye will at short distance see the vertical lines indistinctly, but horizontal lines still distinctly; and therefore near objects will be elongated in a horizontal direction. The portrait-painter, in whom a slight degree of astigmatism manifested itself at first only by the indistinctness of the horizontal lines, has now become far-sighted for vertical lines, and therefore sees a distant person elongated in a vertical direction; his picture, on the contrary, being at a short distance, is seen by him enlarged in a horizontal direction, and is thus painted still more elongated than the subject is seen: so the fault is doubled. I shall be able to show this more clearly by experiments.

The vertical and horizontal lines of this diagram (Fig. 1) are reflected with equal distinctness upon the screen by the spherical apparatus. Those among my audience who have a decided form of astigmatism will, nevertheless, see them differently. Those whose sight is normal will only observe a difference after I have added a cylindrical lens to this apparatus, and thus made it astigmatical (Fig. 2).

FIG.1

FIG.2

Ordinary spectacle glasses are worked by a rotating movement on the surface of a sphere; cylindrical lenses are worked by moving the glass backwards and forwards upon a cylindrical surface. Such glasses produce an optical effect only in one direction. If instead of

white lines I make the experiment with coloured lines, it will show the mixing of colours produced by astigmatism; and if I now turn the axis of the lens, you will observe the effect of different forms of astigmatism.

I show you here a square (Fig. 3): if I added a cylindrical concave glass, with its axis placed horizontally, the square becomes an oblong.

In order to show you how it is possible that the same eye may see an object at too great a distance elongated in a vertical direction, and, on the contrary, one that is too near enlarged in a horizontal direction, I need only place this cylindrical glass before or behind the focus of the apparatus without turning the axis, and you will then see the square, first elongated in a vertical direction (Fig. 4), and then enlarged in a horizontal direction.

Lastly, I show you a portrait. Imagine to yourself that it represents the person whom the astigmatical painter is painting; then, by aid of the cylindrical glass you can form an idea how the painter sees this person.

If I alter the position of the glass, the portrait assumes the form in which the painter sees his own painting on the canvas. This will explain to you why he paints the portrait still longer than he sees the person.

With regard to an anomaly of sight, which seems almost foreign to the subject of painting—I mean colour-blindness—I will also say a few words here, as the subject seems to be regarded with particular interest in England.

What we call colour-blindness is a congenital defect of vision, which is characterized by the absence of one of the three primary sensations of colour. The primary sensations of colour are red, green, and violet, according to Thomas Young and Helmholtz; or

red, green, and blue, according to Maxwell. When, as may easily happen, to this defect is joined a decided talent for painting, drawing alone ought to be attempted, because so absolute a defect will soon assert itself. But we meet with slighter degrees of colour-blindness, where the perception of red is not entirely wanting, but only considerably diminished; so that, for instance, an intense or strongly-illuminated red can be perceived as such, while a less intense red appears green. This moderate degree of colour-blindness does not always deter people from painting. A proof of this I saw at the last year's Exhibition, in a picture which represented a cattle-market. The roofs of the surrounding houses were all painted red on the sunny side, green in the shadow; but—what particularly struck me—the oxen also were red in the sun, green in the shadow. The slighter degrees of this anomaly, in the form of an insufficient perception of colours, have probably been the real cause why several great artists, who have become famous on account of the beauty of their drawing and the richness of their compositions, have failed to attain an equal degree of perfection in colouring.

In opposition to these isolated cases, I have to draw your attention to other cases which happen more frequently, and in advanced age, in consequence of a change in the perception of colours. They do not arise from a deficient function of the nervous apparatus of the eye, but in consequence of a change in the colour of the lens.

The lens always gets rather yellow at an advanced age, and with many people the intensity of the discoloration is considerable. This, however, does not essentially diminish the power of vision. In order to get a distinct idea of the effect of this discoloration, it is best to make experiments with yellow glasses of the corresponding shade. Only the experiment must be continued for some time, because at first everything looks yellow to us. But the eye gets soon accustomed to the colour, or rather it becomes dulled with regard to it, and then things appear again in their true light and colour. This is at least the case with all objects of a somewhat bright and deep colour. A careful examination, however, shows that a pale blue, or rather a certain small quantity of blue, cannot be perceived even after a very prolonged experiment, and after the eye has long got accustomed to the yellow colour, because the yellow glass really excludes it. This must of course exercise a considerable influence when looking at pictures, on account of the great difference which necessarily exists between real objects and their representation in pictures.

These differences are many and great, as has been so thoroughly explained by Helmholtz. Let us for a moment waive the consideration of the difference produced by transmitting an object seen as a body on to a simple flat surface, and consider only the intensity of light and colour. The intensity of light proceeding from the sun and reflected by objects, is so infinitely greater than the strongest light reflected from a picture, that the proportion expressed in numbers is far beyond our comprehension. There is also so great a

difference between the colour of light, or of an illuminated object, and the pigments employed in painting, that it appears wonderful that the art of painting can by the use of them produce such perfect optical delusions. It can of course only produce optical delusions, never a real optical identity; that is to say, the image which is traced in our eye by real objects is not identical with the image produced in our eye by the picture. This is best observed by changing the light. Whoever paints in London has but too frequent opportunities of observing this. A little more or less fog, the reflexion of a cloud illuminated by the sun, suffices to alter entirely the colouring of the picture, while the colouring of natural objects is not changed in the same manner.

Let us now return to our experiment with the yellow glass, and we shall find that it affects our eye very much in the same way as a yellow tint in the light, and therefore modifies natural objects in quite a different degree from pictures. If we continue the experiment for a considerable time, the difference becomes more and more essential. As I said before, the eye becomes dulled with regard to the yellow light, and thus sees nature again in its normal colouring. The small quantity of blue light which is excluded by the yellow glass produces no sensible difference, as the difference is equalized by a diminution of sensibility with regard to yellow. In the picture, on the contrary, there is found in many places only as much blue as is perfectly absorbed by the yellow glass, and this therefore can never be perceived, however long we continue the experiment. Even for those parts of the picture which have been painted with the most intense blue the painter could produce, the quantity of blue excluded by the yellow glass will make itself felt, because its power is not so small with regard to pigments as with regard to the blue in nature.

Imagine now that in the course of years one of the transparent media in the eye of a painter had gradually become yellowish, and that this yellow had by degrees considerably increased in intensity, and you will easily understand the influence it must exercise upon his work. He will see in nature almost everything correctly; but in his picture everything will appear to him yellowish, and consequently he will paint it too blue. Does he not perceive this himself? Does he not believe it if told of it? Were this the case, it would be easy for him to correct the fault, since an artist can paint in a yellower or bluer tone, as he chooses. These are two questions which are easily answered by psychological experience. He does not perceive it himself, because he does not remember that he formerly saw in a different way. Our remembrance with regard to opinions, sensations, perceptions, &c., which have become gradually modified in the course of years—not by any external influence or sudden impression, but by a gradual change in our own physical or mental individuality—is almost *nil*.

He does not believe it—I would not say because an artist rarely

recognizes what others tell him with regard to his works, but because with him as with everyone else, the impressions received through his own eye have a stronger power of conviction than anything else. "Sehen geht vor Sagen" (seeing is believing), says the old adage.

We are almost always conscious of *indistinct* vision, be it in consequence of incorrect accommodation or insufficient power of sight, especially if it is not congenital, but has gradually appeared. But it is extremely difficult and in many cases impossible to convince those of their defect who suffer from *incorrect* vision as to form and colour. They never become conscious of it themselves, even if it is not congenital, and the most enlightened and intelligent among them remain incredulous, or become even angry and offended when told of it. Incorrect perception of form may, however, easily be demonstrated. If in consequence of astigmatism a square appears oblong to anyone, he can measure the sides with a compass; or, what is more simple still, he can turn it so that the horizontal lines are changed into vertical ones, and *vice versâ*, and his own sight will convince him of his error. It is more difficult to demonstrate whether a person sees colours correctly or not. Such glaring mistakes as those produced by colour-blindness can be easily recognized, but faults produced by a diminished sensation of small differences in the shades of colour can only be recognized as such by the fact that the majority of persons with normal vision declare them to be faults. Such, for instance, are deviations produced by an incorrect perception of pigments, which in painting makes itself felt by a constantly recurring *plus* or *minus* of a single colour in the whole picture. It may also show itself by small faults in the rendering of every colour. In discussing this subject with artists, they at once declare these anomalies to represent a school, a taste, a manner, which may be arbitrarily changed. They most unwillingly concede that peculiarities of sight have anything to do with it. It seems to me sometimes as if they considered it in a certain measure a degradation of their art, that it should be influenced by an organ of sense, and not depend entirely upon free choice, intelligence, imagination, and talent.

Thus, to return to the point from which we started, if a painter whose lens becomes yellower begins to paint in a bluer tone, it is said that he has changed his style. The painter himself vehemently protests against this opinion; he thinks that he still paints in his old style, and that he has only improved the tone of his colour. His earlier works appear to him too brown. To convince him of his error it would be necessary to remove his lens suddenly. Then everything would appear to him too blue, and his paintings far too blue. This is no hypothesis, but a fact. Patients on whom I have operated for cataract, very often spontaneously declared, immediately after the operation, that they saw everything blue; in these cases I invariably found their crystalline lens to be of an intense yellow colour. In pictures painted after the artists were considerably over sixty, the effect of the yellow lens can often be studied. To me their pictures

have so characteristic a tone of colour, that I could easily point them out while passing through a picture-gallery. As a striking example I will only mention Mulready. It is generally stated that in his advanced age he painted too purple. A careful examination shows that the peculiarity of the colours of his later pictures is produced by an addition of blue. Thus, for instance, the shadows on the flesh are painted in pure ultramarine. Blue drapery he painted most unnaturally blue. Red of course became purple. If you look at these pictures through a yellow glass, all these faults disappear: what formerly appeared unnatural and displeasing is at once corrected; the violet colour of the face shows a natural red; the blue shades become grey; the unnatural glaring blue of the drapery is softened. To make the correction perfect, the glass must not be of a bright gold colour, but rather of the colour of pale sherry. It must be gradually darkened in accordance with the advancing age of the painter, and will then correspond exactly with the colour of his lens. The best proof of the correctness of this statement is, that the yellow glass not only modifies the blue in Mulready's pictures, but gives truthfulness to all the other colours he employed. To make the proof complete, it would be necessary to show that by the aid of yellow glass we saw Mulready's pictures as he saw them with the naked eye; and this can be proved. It happens that Mulready has painted the same subject twice,—first in 1836, when he was fifty years of age and his lens was in a normal state, and again, in 1857, when he was seventy-one, and the yellow discoloration had considerably advanced. The first picture was called, when exhibited, "Brother and Sister; or, Pinching the Ear;" the second was called "The Young Brother." In both pictures a girl, whose back only is visible, is carrying a little child. A young peasant, in a blue smock-frock, stands to the right and seizes the ear of the child. The background is formed by a cloudy sky and part of a tree. Both pictures are in the Kensington Museum. The identity of the composition makes the difference in the colouring more striking. If we look at the second picture through a yellow glass, the difference between the two almost entirely disappears, as the glass corrects the faults of the picture. The smock-frock of the boy no longer appears of that intense blue which we may see in a lady's silk dress, but never in the smock-frock of a peasant. It changes into the natural tint which we find in the first picture. The purple face of the boy also becomes of a natural colour. The shades on the neck of the girl and the arms of the child, which are painted in a pure blue, look now grey, and so do the blue shadows in the clouds. The grey trunk of the tree becomes brown. Surprising is the effect upon the yellowish green foliage, which, instead of appearing still more yellow, is restored to its natural colour, and shows the same tone of colour as the foliage in the earlier picture. This last fact is most important to prove the correctness of my supposition. My endeavour to explain it became the starting-point of a series of investigations to ascertain the optical qualities of the pigments used in painting, and thus to enable us to

recognize them by optical contrivances, when the vision of the naked eye does not suffice to analyze the colours of a picture.

When I had the pleasure of showing this experiment with Mulready's pictures to Professor Tyndall, he drew my attention to the fact that one single colour, namely, the blue of the sky, was not affected by the yellow glass. The blue of the sky was almost the same in both pictures. I could not at once explain the cause of this, but I discovered it afterwards. The fact is, it is impossible to change the sky-blue of the first picture so as to form a colour that looks like it when seen through a yellow glass. If more white is added, the sky becomes too pale; if a deeper blue is used, it becomes too dark. Mulready was thus forced to content himself by giving to the sky in his later pictures the same colour as in the earlier ones.

If we look at Mulready's earlier works through the same yellow glass, they lose considerably in beauty of colouring: the tone appears too weak; the shadows brown; the green, dark and colourless; we see them as he saw them, and understand why he became dissatisfied with them and changed his colouring.

It would be more important to correct the abnormal vision of the artist, than to make a normal eye see as the artist saw when his sight had suffered. This unfortunately can only be done to a certain extent.

If it is the dispersion of light which, as in Turner's case, alters the perception of nature, it can be partly rectified by a kind of diaphragm with a small opening (Donders' sthenopëical spectacles).

In cases of astigmatism, the use of cylindrical glasses will completely correct the aspect of nature, as well as of the picture. Certain anomalies in the sensation of colour may also be counteracted to some extent by the use of coloured glasses; for instance, by a blue glass, when the lens has become yellow, as in Mulready's case.

If science aims at proving that certain works of art offend against physiological laws, artists and art-critics ought not to think that by being subjected to the material analysis of physiological investigation, that which is noble, beautiful, and purely intellectual will be dragged into the dust. They ought, on the contrary, to make the results of these investigations their own. In this way art-critics will often obtain an explanation of the development of the artist, while artists will avoid the inward struggles and disappointments which often arise through the difference between their own perceptions and those of the majority of the public. Never will science be an impediment to the creations of genius.

[R. L.]

WEEKLY EVENING MEETING,

Friday, March 15, 1872.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

John Evans, Esq. F.R.S. F.S.A. &c.

On the Alphabet and its Origin.

The subject of the Alphabet and its Origin is one which has attracted the attention of many observers, and must indeed at some time or other have forced itself on the consideration of nearly all thoughtful minds.

What is the meaning of those six-and-twenty symbols which serve to render our language visible? Why have they assumed the forms in which we now find them? and whence have they been derived to us? These are questions which most of us must have asked, and many of us may have attempted to answer.

Gesenius, de Vogüé, and Lenormant on the Continent; Professor Hewitt Key, Professor Rawlinson, and Mr. E. B. Tylor in England, as well as others, have done much to throw light on this field of research, and have left but little room for after-comers to add to the stock of information on the subject.

The questions connected with it appear to divide themselves under three heads:—

1. As to the origin of writing and the method of its development in different parts of the globe.
2. As to the original Alphabet from which that in common use amongst us was derived: and
3. As to the history and development of that original Alphabet.

The art of writing is that by which, as Bacon says, "the images of men's minds remain in books for ever, exempt from the injuries of time, because capable of perpetual renovation." It is that by which human knowledge has become cumulative, so that the stores acquired during one generation are handed down to those which succeed; and is indeed one of the most important characteristics which distinguish civilized from savage races of man.

So mysterious does this power of conveying information to others, however remote, appear to savages, that they regard written documents as possessed of powers no less than magical, and have been known to

hide them at the time of committing a misdeed which they feared might be discovered by their means. Yet many of those in the lower stages of civilization have some ideas as to pictorial records.

The cave-dwellers of the south of France at a time when the use of metals was unknown, and when reindeer formed one of the principal articles of food in that part of the world, possessed considerable powers of drawing and of sculpture. On some of their bone instruments figures of animals are engraved, which possibly may to the original owners have conveyed some reminiscences of scenes they had witnessed when hunting. Among the Esquimaux such records are frequently carved on their weapons, and the taking of seals and the harpooning of whales are often depicted. Capt. Beechey says that he could gather from these representations a better insight into the habits of the people than could be obtained from any signs or other intimations.

Among the North American Indians the system of picture writing has been more fully developed, and numerous instances are recorded in Schoolcraft's 'Indian Tribes.' A census roll of 1849 gives the details of 34 families comprising 108 souls, by means of symbols for the names of families, such as Catfish, Beaverskin, &c., with marks below showing the number of individuals in each. Records of the events of a deceased warrior's life are often given on his tombstone in much the same manner. The *totem* of his tribe, such as the Reindeer or the Crane, is reversed to show that he is dead; there are marks recording his war parties and wounds, the number of enemies he has killed, or the eagles' feathers he has received for bravery. Even love and war songs are symbolized by a kind of pictorial *memoria technica*, and the record of a night's encampment with details of a party of sixteen, how they had supped, and what they had for supper, has been depicted on a small scrap of birch-bark.

In Mexico, the art of pictorial representation had at the time of the Conquest been carried to great perfection. The bulk of the pictures, however, merely represent wars, migrations, famines, and scenes of domestic life. They were, moreover, able to record dates by means of an ingeniously-devised cycle, and had some idea of attaching a phonetic value to their symbols. Thus the name of Itz-coatl, the fourth king of Mexico, is found represented by a snake with knives of obsidian issuing from its back—the reason being, that the word Itzli meant knives of obsidian, and Coatl meant snake. The same name was also symbolized by the representation of a knife, a pot and water, which shows an approach to a syllabic system of symbols. For the names of the objects, if given at length, would form Itzli-Comitl-Atl, so that the pot—Comitl—would appear in composition merely to have represented Co—. At a somewhat later date, we find the words Pater Noster represented by a flag, a stone, a prickly pear, and a stone. Pantli being a flag, Tetl a stone, and Nochtli a prickly pear. Here also the first and third symbols appear in composition to have been monosyllabic, and the Aztec version of the Latin seems to have

been Pan-tetl Noch-tetl. What might have been the results of the development of such a system, we shall never know, as it was brought to a close by intercourse with Europeans.

In Peru, though some sort of hieroglyphic writing appears to have been known, the chief substitute for writing was the Quipu or knotted cord. This consisted of a main cord with strings of different colours and lengths attached. The colour, the mode of making the loops, knots, or tufts, their distance from the main cord or from each other, had all of them their meaning. Each Quipu had its own keeper or interpreter, and by their means all public accounts were kept. The Wampum in North America was of somewhat similar character, and in Polynesia also the same sort of Quipu is in use. One kept by the principal tax-gatherer in Hawaii, is a knot of cord of 400 or 500 fathoms in length, subdivided again and again for the different districts and families.

There is a tradition among the Chinese of a similar system of recording events by means of a knotted cord having been in use among them previous to the invention of writing. The Chinese system of writing, though far superior to that of the Mexicans, is still not alphabetical but syllabic. At the outset, the characters seem to have been pictorial, but the representations of the objects have now become so much conventionalized and changed, partly in consequence of the method of writing by means of a brush, that there is much difficulty in recognizing them. In the characters representing the words Sun or Day, Moon, Door, Carriage, Boy, the original pictorial origin is evident, as indeed it is in several other instances.

In some cases compound characters are formed by the junction of others of a simple kind. The Sun and Moon together represent the word *Ming*, bright or clear; Water and Eye together symbolize tears.

With a monosyllabic language, the words of which are of necessity limited in number, one sound has often to represent more than one sense, and the Chinese characters have therefore been divided into phonetics or radicals—those which give the sound,—and the classificatory or determinatives, or those which give the sense.

Thus the sign for *a door* with the determinative *an ear*, means *to listen;* with that of *a corpse* or of *the heart*, means *sorrow*, &c.

The Egyptian hieroglyphics present much analogy in character with the Chinese method of writing. In their earliest form they seem to have been principally pictorial, though also at the same time symbolic. We find, for instance, that the representation of the vault of heaven, with a star suspended from it, typifies darkness or night; that the arms of a man holding a spear and shield are the symbol of to fight, and that thirst is typified by a calf running. The next stage would appear to have been syllabic, when a certain sign represented a syllable, though often with a second more truly literal sign affixed, denoting the final consonant of the syllable. To prevent mistakes, the signs representing words were often accompanied by other signs, which were merely determinative of the meaning. Thus three horizontal zigzag lines

representing water, showed that the previous symbol designated something connected with liquids—or two legs walking, that the word bore reference to locomotion. Many hieroglyphics, however, appear to be purely literal, though in the case of consonants often having some vowel-sound implied. These literal hieroglyphics stand for the initial letters of the objects or ideas they represent. For instance, a goose flying is the equivalent of P, the initial of *Pai*, to fly; an owl stands for M, the first letter of *Mulag*, the Egyptian name of the bird.

The more careful pictorial representations of the objects such as are to be seen in sculptured hieroglyphics and in formal inscriptions, required, however, too much time for their execution to be adopted as an ordinary means of writing. In consequence, the signs became conventionalized, and the salient characteristics of the object were seized on for the more cursive form of writing known as the hieratic. From this again was derived the writing known as demotic, in which many of the symbols have become so much changed and simplified, that it is with difficulty that they can be identified as descendants of originally pictorial forms.

A modified form of hieroglyphic writing is still in use among us, more especially in connection with the science of astronomy; and the conventional forms which now represent the signs of the Zodiac are very instructive as to the amount of modification such symbols are liable to undergo.

In Aries (♈) and Taurus (♉) the heads of the ram and the bull may still be recognized. Gemini is represented by the twin straight lines, ♊; Cancer by its claws, ♋; and Leo by its head and tail, ♌. In the symbol for Virgo there appears to have been some confusion between Astræa and the Virgin Mary, the sign being symbolized by the letters ♍ M. The scales of Libra, the sting of Scorpio, and the arrow of Sagittarius, can still be traced in the symbols, ♎, ♏, ♐. The twisted tail of Capricornus survives in ♑, and Aquarius is represented by two wavy lines of water, ♒. The remaining sign of Pisces has been much metamorphosed, but the two fishes, back to back, with head and tail alternating, can readily be reconstructed from the symbol ♓.

The gradual simplification of form exhibited in these signs, and in the Chinese and hieratic systems of writing, must be borne in mind when studying the development of other systems.

With regard to the origin of the alphabet in common use in Europe there can be no doubt; the testimony of classical historians, as well as that of the letters themselves, being conclusive as to its Phœnician source. The Greek myth of letters having been introduced by Cadmus the Phœnician, seems simply to embody this truth, for there is much probability in the view which connects the name of Cadmus with the Semitic word Kedem, the East.

At what date letters were first in use in Greece is by no means certain, but Grote thought that they were absolutely unknown in the days of Homer and Hesiod (B.C. 850-776). It seems, however,

probable that they were introduced at a somewhat earlier date. If the date which has been assigned to the famous "Moabite stone," of about 900 B.C., be correct, the correspondence in form between the archaic Greek letters and those on the stone raises a strong presumption in favour of letters having been imported into Greece at the time when the Phœnician alphabet was in that stage of development in which it occurs on the stone.

Even the name of the alphabet preserves the memory of its Phœnician origin, for Alpha and Beta, the names of the two letters from which the word is derived, are not really Greek, but merely the Hellenized forms of the Phœnician Aleph and Beth. The same is the case with the names of all the other Greek letters down to Tau; the last five letters, Υ, Φ, Χ, Ψ, Ω, being of later introduction.

The correspondence in form between the Roman, the Greek, and the early Phœnician alphabet, as given on the Moabite stone, can readily be traced. It must, however, be remembered that the letters of the latter are written from right to left, or in the same manner as Hebrew, and not, as is the case with us, from left to right. In the early Greek inscriptions it appears to have been a matter of indifference in which direction the letters were placed. In some the lines are alternately in either direction, and this form of writing was known as Boustrophedon, or that which turned backwards and forwards like an ox in ploughing.

In tracing the correspondence between the Roman, Greek, and Phœnician alphabets, but little need be said with regard to most of the letters.

A.—Alpha, or Aleph, was at the outset, like most of the letters which have now become vowels, rather a representative of a vowel-sound than of an absolute vowel.

B.—Beta, or Beth, has to a great extent preserved its sound, though originally softer and more like V.

C.—Gamma, or Gimel, affords an illustration of change in the power of a symbol, the Greek Γ having become the Latin C. There are indeed several words which until a comparatively late date were written indifferently with a C or a G, as Caius and Gaius; Cneius and Gneius.

D.—Delta, or Daleth, requires no comment.

E.—Epsilon, or He, was originally an aspirate, but, like some other aspirates to which probably some vowel-sound was as it were attached, it gradually softened into a vowel.

F, or Vau. This letter has dropped out of the Greek alphabet, but is found on coins and early inscriptions as the Bau or Digamma, and as the representative of the numeral 6.

G.—Zeta, or Zain. It may seem strange that the Greek Z is represented by the Latin G, a letter which is said to have been introduced by Carbilius. The sound, however, of the Greek Z appears to have been somewhat like Ds, which readily slides into a soft G. One form of the Greek Zeta, ζ, corresponds closely with an early form of G.

H.—Eta or Cheth affords another instance of an aspirate becoming a vowel in Greek, though it retains its aspirate character in Latin, as indeed it did in some early Greek inscriptions.

—Theta or Teth has disappeared in the Latin, though it was retained in the Etruscan alphabet, and moreover is found as a barred D Ð on ancient British and Gaulish coins, and reappears in Saxon as **Ð**.

I. J.—Iota or Jod. This letter, from the name of which our word jot is derived, though originally as large as any of the other letters, becomes the smallest in some alphabets, such as those in use on Cilician coins of the fourth century, B.C., and on *stélés* and *papyri* from Egypt of the first three centuries B.C. It was about that time that the square characters in which Hebrew is now written were gradually being developed from the older forms of letters.

K.—Kappa or Caph. In Latin words this letter only occurs joined with A, as if possibly the original form had been syllabic. In the same manner Q occurs only with U.

L.—Lambda or Lamed.

M.—Mu or Mem.

N.—Nu or Nun, requires no comment. It has been supposed by some that the word *elementa*, occasionally applied as the Latin name for letters, was compounded from these L.M.N. Such a derivation is, however, by no means certain.

—Xi or Samech. This letter is unknown in the Latin alphabet. The correspondence in form of the early Greek Ξ and the Samech of the Moabite stone shows their identity; though Gesenius thought that the Greek Sigma was derived from the Samech, and that when the Sigma coalesced with the Doric San—the equivalent of Shin, Ϡ was put in the vacant place.

O.—Omicron or Ain, like some other vowels, was originally an aspirate, or rather a strong guttural.

P.—Pi or Pe are the same letter, though the second stroke of Π was lengthened to distinguish it from Γ and P.

—Tsade appears never to have come over into the Greek or Latin alphabets.

Q.—Koppa or Koph has become obsolete in Greek, though found on coins and in inscriptions, and retained as the numeral for 90.

R.—Rho or Resh has in Greek retained the form of P, from which in Latin it was necessary to distinguish it by a tail. The same necessity did not exist in Greek, where the Latin P is represented by Π.

S.—Sigma or Shin. This letter has in Latin entirely lost its angularity, while in Greek the Σ was placed vertically instead of horizontally, as in the Phœnician, on account of its resemblance to M.

T.—Tau or Tau. With this letter the original alphabet terminates.

The other letters of our alphabet are in some cases, like X, almost superfluous, in others, like U and Y, different forms of the same Greek letter; and in the case of Z, an old form reintroduced with a fresh value.

As to the original identity of the three alphabets which have been

discussed, there can be no doubt; neither can any exist as to the order in which the letters were originally arranged. For in the Hebrew Scriptures, the language of which may practically be regarded as the same as the Phœnician, there are several instances in which a succession of passages, each commencing with a different letter of the alphabet, present them in this order. A well-known example is afforded by the 119th Psalm, each of the twenty-two sections of which commences with a different letter, the name of which forms the heading to each in the English version of the Bible.

When, however, we come to consider the history and development of the Phœnician alphabet, we are no longer upon so sure a footing. The manner in which some other forms of writing, such as the Chinese and the Egyptian hieratic were developed, will have prepared us for the probability of the Phœnician alphabet having also been evolved from a pictorial source, and for finding in it a similar history to that recorded in the nursery rhyme—

A was an Archer and shot at a frog,
B was a Butcher and had a big dog.

It is a by no means unimportant fact in reference to this view, that the names of the Phœnician or Hebrew letters are not arbitrary, but each significant of some object; though the meaning of the names cannot in all cases be recognized with absolute certainty. For instance, Aleph, Beth, Gimel, and Daleth, mean Ox, House, Camel, Door, and if we find that these and the succeeding letters, when in their most primitive forms (so far as known) present similarities with the whole or a portion of the objects by the names of which they are distinguished, there is a strong probability of a pictorial origin for the letters.

Taking the forms of the letters, as given on the Moabite stone, in conjunction with the meaning of their names, such a similarity can in all cases be traced, though more certainly intentional in some letters than in others. This will be best shown in a tabular form. *

א Aleph.	An ox	*The head of an ox.*—That this letter was known to embody this symbol is recorded by Hesychius about A.D. 380. The correspondence of a small a or *a* with the sign for Taurus when placed horizontally is worth notice, ∝.
ב Beth ..	A house, or possibly a tent.	*A house, showing one wall and the ridged roof.*
ג Gimel	A camel	*The head and neck of a camel.*
ד Daleth	A door	*The triangular door of a tent.*
ה He ..	A lattice or window	*A lattice?*—The meaning of the name of this letter is somewhat doubtful.
ו Vau ..	A peg or nail	*A peg.*
ז Zain ..	A weapon	*An arm holding a spear?*
ח Cheth	An enclosure, or field	*An enclosure.*—Much like the Chinese figure for the same meaning.

* The letters are here given in the ordinary Hebrew character instead of the older form.

ט Teth ..	A serpent	*A coiled snake.*—This letter does not occur on the Moabite stone.
י Jod ..	The hand	*The hand and wrist in profile*, similar to what may be seen on some early Hindu coins.
כ Caph ..	The palm of the hand.	*An open hand*, as in some drawings of the North American Indians.
ל Lamed	An ox-goad	*An ox-goad?*—The meaning of the name somewhat doubtful.
מ Mem ..	Water	*A wavy line.*—Like the representation of water on early coins and sculpture, and as in the sign Aquarius ♒.
נ Nun ..	A fish	*The head, gill, and back of a fish.*
ס Samech	A support	*A kind of prop supporting a trellis for vines.*—Mr. Hensleigh Wedgwood has pointed out the similarity of this letter to the figure of a sculptor's bench or easel in Egyptian pictures.
ע Ain ..	The eye	*The pupil of the eye*, as in Egyptian hieratics.
פ Pe ..	The mouth	*The two lips open at an angle*, much like the mouth as represented on some ancient British coins.
צ Tsade	A reaping hook ..	*A reaping hook or scythe attached to its handle.*
ק Koph	The back of the head?	*The head and neck?*
ר Resh ..	The head	*The head in profile.*
ש Shin ..	A tooth	*A tricuspid tooth.*
ת Tau ..	A mark	*A cross*, like the mark still made by those who cannot write.

This correspondence in form can hardly be appreciated without diagrams, but in many instances is striking, and in none absolutely forced. There have, however, been numerous objections raised to such a view of the derivation of the forms of the Phœnician letters.

Lenormant and de Rougé would rather trace them to Egyptian hieratic characters, but the resemblances they point out between them are but slight, and in no instance does the Phœnician name of the letter agree with that of the object represented by the Egyptian hieratic. Moreover the resemblances, when traced, are rather with later forms of Phœnician letters than with those on the Moabite stone.

Mr. E. B. Tylor also considers that the theory maintained by Gesenius of the Phœnician letters being pictorial, can be shown to be unsafe. He thinks the resemblances between the letters and the objects to be but small, and the bond which attaches the name to the letter to be but slight; that the coincidences are not primary and essential, but secondary and superficial. In support of this view he instances the old Slavonic alphabet, and the Runic *Futhorc*, in which the letters have names unlike those of our alphabet, but each with a meaning—the initials of the names giving the power of the letters. He suggests that in a similar manner Hebrew words may have been

chosen as names for letters derived from some extraneous source, such names having the proper initial letter, and also some suitability to describe its shape—the same as if in English we called

A—Arch or Arrowhead.

B—Bow or Butterfly.

C—Curve or Crescent.

This, however, is contrary to all analogy among methods of writing of which we know the development, and moreover, several of the names of the Hebrew letters are not actual words in common use in the Hebrew writings, but words which have become obsolete, and of which in one or two cases it is hard to recover the meaning. The letters, moreover, cannot originally have been mere arbitrary signs, or there would have been greater distinctions between some of them, such as it was subsequently found desirable to introduce.

If too, the Phœnician letters came from an extraneous source, we may well ask where it was, and how does it happen that no traces of the original names of the letters have been preserved. In the Greek alphabet, which is undoubtedly derivative, the names of the letters would alone suffice to show the source from which it came; and the case of the Runic alphabet, derived from the same source, though with the letters rearranged and with new names given at a comparatively recent date, seems hardly to apply. The Runic names, moreover, exhibit no attempt to denote the forms of the letters, to which they are as inapplicable as the names in one of the Irish alphabets, in which each letter is called by the name of some tree.

It seems, on the contrary, far more probable that the Phœnicians, possibly in the first instance borrowing the idea from the Egyptians, struck out for themselves a more purely literal and therefore a more simple and useful alphabet. A classification of sounds once established, and a system of syllabic symbols once invented, the transition to a pure literal alphabet is comparatively easy, especially when once the syllabic symbols have, from the introduction of foreign words or from other causes, been employed for the initial sound only of the syllables they represent.

Such a change, involving a departure from old practice, might perhaps more readily take place in an adjacent country to that in which the syllabic system prevailed, than in the country itself; and we may readily conceive a practical people like the Phœnicians importing from Egypt a system of pictorial writing thus modified.

Certainly their alphabet, unlike the letters of the later class of Egyptian hieroglyphics, does not appear to consist of merely a few survivors from a whole army of symbols. On the contrary, it seems to present some traces of arrangement; for the objects representing the letters appear to be grouped in pairs, each comprising two objects in some manner associated with each other; and between each pair is inserted a third letter, represented by an object not so immediately connected with those preceding it, but still not absolutely alien from them.

Thus the ox and the house are followed by the camel—an animal, by the way, not represented in Egyptian hieroglyphics. The door and the window are followed by the peg; the weapon and enclosure by the serpent; the hand and the palm by the ox-goad; the water and the fish by the support; the eye and the mouth by the reaping-hook; the head and the back of the head by the tooth; and the alphabet concludes with the final mark, ×.

It would be superfluous to attempt to point out the bearings of this question of the origin and development of the Phœnician alphabet on the history of civilization in Europe and Western Asia.

Future discoveries may possibly bring us nearer the cradle of this alphabet, but it seems probable that on the Moabite stone we find the letters still retaining enough of their original pictorial character to justify a belief that they there occur in a comparatively early stage, and not removed by many centuries from the time when they were merely delineations of the objects, the names of which they have preserved. Assuming this to have been the case, what is the stage of culture to which the inventors of this alphabet appear to have attained?

They were not mere nomads or hunters, but a people with fixed dwellings for themselves and enclosures for their cattle. They were acquainted with agriculture, and had domesticated animals, and employed the ox as a beast of draught to cultivate fields, the produce of which they reaped with metallic sickles. In fact, their civilization would seem to have been at least equal to that of the bronze-using people of the Swiss lake-dwellings.

[J. E.]

WEEKLY EVENING MEETING,

Friday, March 22, 1872.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

J. NORMAN LOCKYER, Esq. F.R.S. M.R.I.

On the Results of the latest Eclipse-Expedition.

[Abstract deferred.]

GENERAL MONTHLY MEETING,

Monday, April 1, 1872.

Sir HENRY HOLLAND, Bart. M.D. D.C.L. President, in the Chair.

Louisa Lady Ashburton,
Charles Balme, Esq.
Mrs. W. Clement Cazalet,
Francis Halhed Ward-Jackson, Esq.
Samuel Morley, Esq. M.P.
S. J. Smith, Esq. F.G.S. F.C.S.
Charles Whittingham, Esq.

were *elected* Members of the Royal Institution.

The following Address of Congratulation to Her Majesty the Queen, to be signed by the President on behalf of the Members, was read and approved by the Meeting.

"TO THE QUEEN'S MOST EXCELLENT MAJESTY.

"MADAM,

"We Your Majesty's most dutiful subjects, the President, Managers, Visitors, Professors, and Members of the ROYAL INSTITUTION OF GREAT BRITAIN for the Promotion, Diffusion, and Extension of Science and Useful Knowledge, humbly offer our heartfelt congratulations to Your Majesty on the Recovery of His Royal Highness the Prince of Wales from His dangerous illness, which caused such universal and intense anxiety; and we desire to express our earnest wish, that the valuable life of His Royal Highness may be long preserved for the happiness of Your Majesty and His family, and for the lasting benefit of the nation."

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Governor-General of India—Memoirs of the Geological Survey of India:—
Palæontologia Indica, Series VI. Parts 9–13. fol. 1871.
Records, Vol. IV. 3, 4. 8vo. 1871.
W. T. Blanford, Observations on the Geology and Zoology of Abyssinia. 8vo. 1871.

Director of the Geological Survey—Mineral Statistics for 1870. 8vo. 1872.

American Philosophical Society—Proceedings, No. 87. 8vo. 1870.
Transactions, Vol. XIV. Part 3. 4to. 1871.

Astronomical Society, Royal—Monthly Notices, Vol. XXXII. Nos. 3, 4. 1872.

Bacon, G. Mackenzie, M.D. (the Author)—On the Writing of the Insane (with Illustrations). 8vo. 1872.

British Architects, Royal Institute of—Sessional Papers, 1871–2. Nos. 6, 7. 4to.

British Association for the Advancement of Science—Report of the Forty-first Meeting held at Edinburgh, August, 1871. 8vo. 1872.

British Museum Trustees—Guide to Greek Coins. 8vo. 1872.

Chemical Society—Journal for Feb. 1872. 8vo.

Editors—Academy for March, 1872. 4to.
American Journal of Science for Feb. 1872. 8vo.
Artizan for Feb. 1872. 4to.
Athenæum for Feb. 1872. 4to.
Chemical News for Feb. 1872. 4to.
Engineer for Feb. 1872.
Food Journal Jan.-March, 1872. 4to.
Horological Journal for March, 1872. 8vo.
Journal of Gas-Lighting for March, 1872. 4to.
Mechanics' Magazine for March, 1872. 8vo.
Nature for March, 1872. 4to.
Pharmaceutical Journal for March, 1872. 8vo.
Photographic News for March, 1872. 4to.
Geographical Society, Royal—Proceedings, Vol. XV. No. 5. Vol. XVI. No. 2. 8vo. 1871-2.
Hayden, F. V. Esq. (the Author)—Hot Springs and Geysers of the Yellow-stone and Firehole Rivers (from American Journal of Science, Vol. III. 1872).
Linnean Society—Journal, No. 66. 8vo. 1872.
London Institution—Journal, No. 11. 8vo. 1872.
Macpherson, John, M.D. M.R.I. (the Author)—Annals of Cholera, from the earliest period to the year 1817. 8vo. 1872.
Manchester Literary and Philosophical Society—Proceedings. Vol. XI. No. 10. 8vo. 1872.
Meteorological Society—Quarterly Journal, New Series, No. 1. 8vo. 1872.
Nichols, J. Gough, Esq. F.S.A. (the Author)—On the three editions of Watson's Memoirs of the ancient Earls of Warren and Surrey. (From Herald and Genealogist.) 8vo. 1872.
Preussische Akademie der Wissenschaften—Monatsberichte, Dec. 1871. 8vo.
Royal Society of Literature—Transactions, Second Series, Vol. X. Part 1. 8vo. 1872.
Royal Society of London—Proceedings, No. 132. 8vo. 1872.
Philosophical Transactions for 1871, Part 2. 4to. 1872.
St. Pétersbourg, Académie Impériale des Sciences—Mémoires, Tome XVI. Nos. 9-14. Tome XVII. Nos. 1-9. 4to. 1870-1.
Bulletins, Tome XVI. Nos. 2-6. 4to. 1871.
Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, March. 1872. 8vo.
Victoria Institute—Journal of Transactions, Vol. V. No. 20. 8vo. 1872.
Würzburg Physikal.-Medecin. Gesellschaft—Verhandlungen, Band I. Band II. Heft 1-2. 8vo. 1871.

WEEKLY EVENING MEETING,

Friday, April 12, 1872.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

JOHN MORLEY, Esq.

On the Influence of Rousseau.

ROUSSEAU'S teaching sowed the seeds of the restlessness which is so manifest throughout modern Europe. The teaching itself was not in its fundamental ideas possessed of great creative originality. Rousseau owed much to Geneva, where he was born, in the midst of public discontents and the discussion of the first principles of politics and theology: for instance, the doctrine of the sovereignty of peoples, and the ten-

dency towards natural religion and a pure deism. He owed much to Montaigne, Plutarch, Hobbes, and Locke. His originality lay in the temper and sentiment which he introduced into social discussions, and which exactly suited the ripeness of the times. Until his memorable diversion, the battle between the old and new in France had been between the Church and the Encyclopædists or Voltaireans. The latter were rationalistic, critical, argumentative, believing enthusiastically in progress by means of new intellectual acquisitions. Rousseau sided with neither school, having no taste for dogmatic authority on the one hand, nor for mere knowledge or controversy on the other. His influence took root in a new type of life, springing from the old appeal away from the corruption and frivolity of artificial society, to simple and pure individual life; dogma being abandoned for spiritual theism, and pride of intellect for health of feeling. Hence his writings enlarged the attack which the philosophers had carefully restricted to the Church and theology, so as to comprehend the whole social order in destructive criticism. They touched the entire circle of life, and hence they inspired not only the Jacobin Robespierre, or the transcendental deist, George Sand, but the royalist Châteaubriand and the Christian Lamennais. They produced ultimately a magnificent expansion of the literature of imagination. But if Rousseau had confined himself to the presentation of a moral idea, his work would have taken a place with the other Utopian visions by which men have been cheered and elevated. He proceeded from man to the *milieu*, from the consideration of the right type of manhood and the means by which we are to be prepared for the right type of life, to the conditions of society. It was his method in the latter aspect which has made him so mischievous. He cut off the whole past of the race, obliterating history from memory and the old social ordering from sight, and inundating the study of social truth with metaphysical *a priori* figments, such as, Rights of Man, Laws of Nature, and the rest. The revolutionary dogma of the equality of man, of the equality of material condition, were both involved in his conception, though with respect to equality of material condition he did not insist on it from any exaltation of the material side of life; on the contrary, his notion of democracy was truly spiritualistic, and had its root in a moral revolt. His fundamental merit was his protest against the stationary fatalism of those who exaggerate the strength of social continuity and the grip of the past over the present. His fatal error was his disregard of the intellectual and moral endeavour of the past, and a blind ignorance of the experience of the race. What made his errors so baneful was the concurrence of the economic and political conditions of France, which were so devoid of all coherence as to permit his anti-social speculations to become violent explosive forces. He deserves, however, the gratitude of mankind for the impulse which he gave to the motives for the study of social truth, by pointing out how short a way we have gone towards the admission of the bondsmen of society to the best advantages of civilization. [J. M.]

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, March 22, 1872.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

J. NORMAN LOCKYER, Esq. F.R.S. M.R.I.

On the Eclipse Expedition, 1871.*

I UNDERSTAND my duty to-night to be to give an account of the observations made, not by all who observed the eclipse of last December, but by the members of the party which went out under the auspices of the British Association, and it is extremely fortunate that nothing more is required of me: first, because most valuable work was done by the other parties, which of itself would require more time to state than I have at my disposal; and secondly, because the amount of material obtained by the members who were sent out from England, and by the friends who met them at every point, is so great, that it would be impossible in one discourse to give anything like an exhaustive account of it. Here are some of the records in this portfolio. You will see at once that even for one party I can only make a selection, and I am perfectly aware of the extreme responsibility which attaches to anyone who may venture to make a selection out of such an enormous mass of material as we have collected.

Before I proceed to discuss the work done by the different parties, it will be desirable to give an idea of the arrangements, and for this purpose I have prepared several maps, which will enable you to see what the British Association parties did.

In the first instance I may remark that the weather conditions were somewhat problematical. Another point of great importance was that much of the ground was fortunately occupied, and it was essential when placing the parties to bear these two considerations in mind—the possibility of bad weather, and then the importance of so arranging matters that if some of the observers were clouded out, belonging to our parties, then the story might be continued by other observers.

Here we have a map of India, which gives you a general idea of

* The chief results obtained by the expedition have been taken from the *ad interim* Report presented to the British Association Meeting at Brighton. The lecture itself dealt mainly with the methods and instruments employed.—J. N. L.

the path of the shadow during the eclipse. The shadow, you see, strikes India on the western coast, and it runs down in a south-westerly direction, and cuts the northern portion of Ceylon.

When we arrived in India we found that the Indian observers, consisting of those well-known men Tennant, Herschel, Hennessy, Pogson, and others, had determined, from their knowledge of the climatic conditions of India at that time of the year, to occupy the central part of the line, and also a station at a low level; the eminent French physicist M. Janssen taking up his position at the top of the Nielgherries. We were to station ourselves either east or west, or both, of these parties. Whether east or west would depend upon the monsoon, and the great question that was being discussed on our arrival was, Was the monsoon favourable?

I have not time to go into the many interesting points touching the answer to this question; but I may say shortly that what we heard was, that if the weather was likely to be bad on the east side of the hill range, generically called the Ghauts, there was a good chance for anyone occupying a position west of those hills. What happened was that we did occupy the positions marked by blue wafers on the map, namely, Bekul on the west coast, Manantoddy on the western slope of the Ghauts, Poodocottah in the eastern plain, and in the island of Ceylon, first, Jaffna; and secondly, Trincomalee.

Such were our arrangements. The parties were stationed along the line of totality. Very different were the arrangements of the Sicilian party of the former year. In Sicily we were compelled to throw ourselves *across* the line of totality in the direction which I have indicated on this map of Sicily.

Now what was the work we had to do? If you will allow me to refer to two or three results of the former Eclipse Expedition, I will endeavour to put them before you without taking up too much of your time.

One of the most important among the results obtained in the eclipse of 1870 was this: far above the hydrogen which we can see every day without an eclipse—far above the prominences, the spectrum of hydrogen had without doubt been observed by two or three of the American observers, who were more fortunate than we were. Among them Professor Young stated, that the spectrum of hydrogen was observed to a distance of 8 minutes from the sun; he then adds, "far above any possible hydrogen atmosphere." This is point number one.

Another of the points was this: the unknown substance which gives us a line coincident, according to Young, with a line numbered 1474 by Kirchhoff, had been observed by the American observers to a height of 20 minutes above the limb of the dark moon.

Now, it was a very obvious consideration that if we got a spectrum of hydrogen 8 minutes from the dark moon, when we thought we knew that the hydrogen at the sun did not really extend more than 10 seconds beyond the dark moon, there was something at work which had the effect of making it appear very much more extensive than it

really was; and it was fair to assume that if this happened in the case of the hydrogen, it might also happen in the case of the unknown stuff which gives us the line 1474.

In support of this view we had one of the few observations which were made in Sicily, in the shape of a drawing of the corona, as seen by Professor Watson, who observed at Carlentini. He saw the corona magnificently; and being furnished with a powerful telescope, he made a most elaborate drawing of it, a rough copy of which I will throw on the screen. You will see at once that we had in this drawing something which seemed to militate against the idea that the 1474 stuff at the sun did exist to a height of 20 minutes. According to Professor Watson the boundary of the real corona was clearly defined, its height being far under that stated.

Next, we had another observation of most important bearing on our knowledge of the base of the corona. I refer to the announcement of the observation by Professor Young of a stratum in which all the Fraunhofer lines were reversed. It was asserted that there was undoubtedly a region some 2 seconds high all round the sun, which reversed for us all the lines which are visible in the solar spectrum. We had in fact in a region close to the photosphere the atmosphere of the sun demanded by Kirchhoff at some distance above the photosphere.

Last, not least, we had the photographic evidence. There was in Sicily a photographic station at Syracuse, and the Americans had another in Spain. I now show on the screen a drawing—it is not the photograph itself—but a drawing of a photograph made by the party in Sicily; what we have on this photograph, is a bright region round the dark moon, which is, undoubtedly, solar, but stretching out right away from this, here and there are large masses of faint light, with dark spaces between them, which have been called rifts. Now the question is, Is this outer portion solar?

Having thus brought rapidly before you some of the questions which we had principally to bear in mind, and, if possible, settle (though that is too much to hope for in any one Eclipse Expedition) in the work we had to do in India, I will next bring to your notice some new methods of inquiry which had been proposed, with the object of extending former observations.

I may here remark that the Royal Astronomical Society, in the first instance, invited me to take charge of an expedition to India merely to conduct spectroscopic observations; but although this request did me infinite honour, I declined it, because the spectroscope alone, as it had been used before, was, in my opinion, not competent to deal with all the questions then under discussion. I have told you that some of the most eminent American observers had come to the conclusion that the spectrum of hydrogen observed in the last eclipse round the sun, to a height of 8 minutes, was a spectrum of hydrogen "far above any possible hydrogen" at the sun. Hence it

was in some way reflected. Now with our ordinary spectroscopic methods it was extremely difficult, and one might say impossible, to determine whether the light which the spectroscope analyzed was really reflected or not; and that was the whole question.

It became necessary, therefore, in order to give any approach to hopefulness, to proceed in a somewhat different way in the 1871 expedition; and in order to guard against failure, to supplement such new observations by photographs; and fortunately we were not long in coming to a conclusion that this might be done with some considerable chance of success.

I have here a train of prisms. I will for one moment take one prism out of the train, and we will consider what will happen if we illuminate the slit of the lantern with a monochromatic light and observe it through the prism. If we render sodium vapour incandescent, we know we get a bright yellow image of the slit, due to the vapour of the metallic sodium only giving us yellow light. But why is it that we get a line? Because we always employ a line for the slit. But suppose we vary the inquiry. If, instead of a straight line we have a crooked line for the slit, then we ought to see a crooked line through the prism. Now, allow me to go one step further: suppose that instead of a line, whether straight or crooked, we have a slit in the shape of a ring, shall we see a ring through the prism? You will see that we shall. And then comes this question: If, when we work in the laboratory we examine these various slits, illuminated by these various vapours, why should it not happen that if we observe the corona in the same way, we shall also get a ring built up by each ray of light which the corona gives to us; since we know, from the American observations, that there were bright lines in the spectrum of the corona, as observed by a line slit? In other words, the corona examined by means of a long train of prisms, should give us an image of itself painted by each ray which the corona is competent to radiate towards us.

Now let us pass to the screen, the screen merely replacing the retina. We will first begin with the straight slit with which you are familiar—we now have our slit fairly focussed on the screen—we then in the path of the beam interpose one of these prisms, and there we get on the screen a bright line.

Now, to continue the argument, we replace the straight slit by a crooked one, and you see we get a crooked image on the screen. We now replace this crooked slit by a ring. We have now a ring-formed image on the screen. So that you see we can use any kind of narrow aperture we choose, and as long as we are dealing with light which is monochromatic, or nearly so, we get an image of the aperture on the screen.

If we consider the matter further it will be evident that we may employ a mixture of vapours, and extend this result.

We will now, for instance, instead of employing sodium vapour, employ a mixture of various vapours. You see now that each ray

given by these substances, instead of building up a line image, is building up for us a ring image—that we have now red, green, yellow, blue, and violet rings.

Now that was the consideration which led to the adoption of one of the new attempts to investigate the nature of the corona used this time. It was, to use a train of prisms, pure and simple, using the corona as the slit, a large number of prisms being necessary to separate the various rings we hoped to see, by reason of their strong dispersion. On the screen the rings to a certain extent intersect each other; and it would have been easier to show you the ring-form of the images if we could have used more prisms than one.

If this is good for a train of prisms such as I have referred to, it is good for a single prism in front of the object-glass of a telescope. Such was the method adopted by Professor Respighi, the distinguished Director of the Observatory of the Capitol of Rome, who accompanied the expedition.

Now you may ask how would this method, if it succeeded, be superior to the ordinary one? In this way. If we were dealing merely with reflexion, then all the rings formed by vapours of equal brilliancy at the base of the chromosphere would be of the same height, while if reflexion were not at work the rings would vary according to the actual height of the vapours in the sun's atmosphere, and the question would be still further advanced if the spectrum did not contain a ring representing the substance which underlies the hydrogen.

Our *new* spectroscopic equipment then was as follows:—

1. A train of five prisms.
2. A large prism of small angle placed before the object-glass of a telescope.
3. Integrating spectroscopes *driven by clockwork*.
4. A self-registering integrating spectroscope, furnished with telescopes and collimators of large aperture, and large prisms. (This instrument was lent by Lord Lindsay.)

Now a word about the polariscopic instruments, referring you to my lecture given last year for a general notion of the basis of this class of observation.

A new idea was that observations to determine the polarization of the corona might be made with the same telescope and eye, both with the Biquartz and the Savart.

By the kindness of Mr. Spottiswoode, who has placed his magnificent polarizing apparatus at our service, I hope to be able to show you on the screen the mode of examining the corona by means of those two instruments, so as to enable you pretty well to follow what was actually done.

Let me begin with the Biquartz polariscope. In the first instance I will throw on the screen a representation of the corona itself, and we will then insert a Biquartz, and see its effect when I flood the

screen with polarized light. You now see an indication of what would be observed supposing the polarization was due to polarized light diffused in the region between us and the dark moon and eclipsed sun, in which case the polariscopic effect would be observed generally over the dark moon, the corona and the region of the sky outside the corona. But this is not all; not only does this arrangement enable us to determine the existence of such a general polarization, but the vertical line in the Biquartz called the line of junction indicates the plane of polarization, when the colours on both sides of it are the same; so that we have two colours strongly contrasted in either half of the field when we are away from the plane of polarization, and a uniform colouring of the whole field when in or at right angles to that plane. By turning this prism through 90 degrees, you see I entirely change the colours.

But we are not limited to the Biquartz in this inquiry. We can apply the Savart polariscope. Having still our image of the corona on the screen, I now replace the Biquartz by a Savart.

We now no longer see a line of junction with the similar or different colours on either side of it, but lines of colour running across the image. I turn the prism. We first see the lines with a white centre, then with a dark one; while at times they are altogether absent. And as a departure from the plane, when we use the Biquartz, gives us the strongest contrasts of colour, so you observe that with the Savart under these circumstances all indications of polarization vanish.

Now, if we assume polarization to be general, and the plane of polarization vertical, we should get those coloured bands, as you see them there, crossing the corona and dark moon, the lines being vertical and dark-centred. If the plane of polarization were horizontal, we should find the lines horizontal and the central one white.

But so far as we have gone, we have been dealing with polarization which is general, and we have not attempted to localize polarization at the corona itself. But I have here an apparatus, by means of which, quietly, in this theatre one can see as admirable an example as we should desire, of polarization, assumed to be particular to the sun and not general—I mean radial polarization. We have simply a circular piece of mahogany, or something else which polarizes light equally well, with a hole in the middle with sloping sides, cut as you see this cut, and then we place behind it a candle, so that the light of this candle, after falling on oiled tissue paper stretched across the aperture, can be reflected to the eye by the sides, the direct light of the candle being stopped by a central metallic diaphragm. We have now a source of polarized light of a different kind from the last. The next thing we have to do is to introduce into a small telescope exactly the same kind of apparatus we have there, though of course on a much smaller scale, and examine the ring of light seen when we put the candle behind the aperture. On examining the ring of light

which is now visible by means of this telescope, which contains a Biquartz and analyzer, I see the most exquisite gradations of colour on either side the line of junction which cuts the field of view and the bright ring in the centre into two.

Now, instead of the candle, we will employ the electric lamp; and instead of the eye, the screen; but I must inform you that the great heat of the electric lamp prevents the appearance being perfectly successful on the screen, as the reflecting varnish is melted.

In this experiment we cannot work with an image of the corona. We must make our corona out of the image of the ring we hope to get on the screen; and then, by employing the Biquartz in the same way as before, instead of getting similar colours on either side of the line of junction, as we did when we were working in the plane of polarization, and getting the greatest contrasts, as we did when we worked 45 degrees away, you observe we get different colours in each part of the ring.

On the screen we now have a highly-magnified image of the hollow cone of iron which I am compelling to reflect the light from the lamp; and by inserting this Biquartz I throw various colours over different portions of that ring, which I beg you to consider for one moment as the solar corona, and the colours change as I rotate this prism. You will at once be able to explain the different actions of this Biquartz in this instance. The reflexion and therefore the plane of polarization is no longer general, but varies from point to point of the reflecting surface. It is in fact radial, and hence the delicate radiate arrangement of colour.

Such then were some of the new methods and new instruments we used for the first time in our researches. And I hope you will allow me to use this term, although our work was conducted a long way from the Royal Institution, the natural home of research in England.

I must now state very briefly some of the results of our work; and first, the certain results.

We were able to make out the structure of the corona. We know all about the corona so far as the structure of its lower brighter strata, that portion, *viz.* which I referred to in my lecture last year as being visible both before and after totality, is concerned. You may define it as consisting of cool prominences; that is to say, if you examine a prominence any day, without waiting for an eclipse, and then go to an eclipse and examine the lower portion of the corona you will find the same phenomena, minus the brightness. You find the delicate thread-like filaments which you are now all so familiar with in prominences—filaments which were first thrown on a screen in this theatre; the cloudy light masses, the mottling, the nebulous structure, are all absolutely produced in the corona, as far as I could see it with a telescope with an aperture of $6\frac{1}{4}$ inches; and I may add that the portion some five minutes round the sun, reminded me forcibly in

parts of the nebula of Orion, and of that surrounding η Argus, as depicted by Sir John Herschel, in his Cape observations.

We have shown that the idea that we did not get hydrogen above 10 seconds above the sun is erroneous; for we obtained evidence that hydrogen exists to a height of 8 or 10 minutes at least above the sun; and I need not tell you the extreme importance of this determination. One of the proofs we have of that lies in this diagram, showing the observations made by Professor Respighi, armed with an instrument, the principle of which I hope you are now familiar with.

Just after the sun disappeared Professor Respighi employed this prism to determine the materials of which the prominences which were then being eclipsed were composed; and he got the prominences shaped out in red, yellow, in blue, and in violet light; a background of impure spectrum filling the field, and then as the moon swept over the prominences these images became invisible; he saw the impure spectrum and the yellow and violet rings gradually die out, and then three bright and broad *rings* painted in red, green, and blue, gradually form in the field of view of his instrument; and as long as the more brilliant prominences were invisible on both sides of the sun, he saw these magnificent rings, which threw him in a state of ecstasy. And well they might.

These rings were formed by C and F, which shows us that hydrogen extends at least 7 minutes high, for *had we not been dealing with hydrogen we should have got a yellow ring as well*, because the substance which underlies the hydrogen is more brilliant than the hydrogen itself, and in addition to the red ring and the blue ring, which indicate the spectrum of hydrogen, he saw a bright green ring, much more brilliant than the others, built up by the unknown substance which gives us the Kirchhoff line, 1474.

Now at the time that Professor Respighi was observing these beautiful rings by means of a single prism and a telescope of some four inches aperture, some 300 miles away from him—he was at Poodocottah and I was at Bekul—I had arranged the train of prisms which you see here so that the light of the sun should enter the first prism, and after leaving the last one should enter my eye. And what I saw is shown, side by side with Respighi's observations, in this diagram, in which I have separated the rings somewhat, so that there should be less confusion than in the actual observation. Here is Professor Respighi's first observation. He gets indications of C, D^3, F, and the hydrogen line near G. He was observing the very lowest, brightest region of all, and therefore 1474 was obliterated by the brightness of the continuous spectrum; but as the eclipse went on D^3 was entirely obliterated, and afterwards he got C and F building up rings together with 1474, which was not represented in the lower regions of the prominence—not because it was not there, but because, as I have already insisted, of the extreme brilliancy of the background. Now my observation was made intermediately, as it were,

between the two observations of Professor Respighi's. Let me show the observations together.

Respighi	..	C	D^3	F	G	Prominences at beginning of eclipse.
Lockyer	..	C	1474	F	G	Corona at 80 seconds from commencement.
Respighi	..	C	1474	F		Corona at mid eclipse.

Note that I had no object-glass to collect light, but that I had more prisms to disperse it; so that with me the rings were not so high as those observed by Respighi, because I had not so much light to work with: but such as they were I saw them better because the continuous spectrum was more dispersed, and because, with my dispersion, the rings—the images of the corona—therefore did not so much overlap. Hence doubtless Respighi missed the violet ring which I saw, so faint, however, that both that and 1474 were almost invisible, while C shot out with marvellous brilliancy, and D^3 was absent.

These observations thus tend to show, therefore, that instead of the element—the line of which corresponds with 1474—existing alone just above the prominences, the hydrogen accompanies it to what may be termed a great height above the more intensely heated lower levels of the chromosphere, including the prominences in which the lower vapours are thrown a greater height. With a spectroscope of small dispersion attached to the largest mirror of smallest focus which I could obtain in England, the gaseous nature of the spectrum, as indicated by its *structure*, that is bands of light and darker intervals as distinguished from a continuous spectrum properly so called, was also rendered evident.

These are results of the highest importance, which alone are worth all the anxiety and labour connected with the expedition.

But there is more behind.

The photographic operations (part of the expense of which was borne by Lord Lindsay) were most satisfactory, and the solar corona was photographed to a greater height than it was observed by the spectroscope, and with details which were not observed in the spectroscope.

Mr. Davis was fortunate enough to take an admirable series of five photographs at Bekul, and Captain Hogg also obtained some at Jaffna; but I am sorry to say the latter lack somewhat in detail.

I have prepared two lamps, because I am anxious to exhibit the photographs two at a time, that you may compare one with the other. [This was done.] You see that so far as the camera goes—and mark this well—the corona was almost changeless during the whole period of totality; this is true, not only for one place, but for all the places at which it was photographed.

I now exhibit two other photographs—one taken at Jaffna and the other at Ootacamund. Actinically the corona was the same and practically changeless at all the stations. You see that, though not so obvious as in the other case, there is the same similarity.

Before I leave the actinic corona, I am anxious to show you an image of it, taken during the American eclipse of 1869 in a camera exposed to the sun during the whole of the totality; to a certain extent in our recent photographs we have reproduced what was photographed in 1869.

The solar nature of most, if not all, of the corona recorded on the plates is established by the fact that the plates, taken in different places, and both at the beginning and end, of totality, closely resemble each other, and much of the exterior detailed structure is a continuation of that observed in the inner portion independently determined by the spectroscope to belong to the sun.

While both in the prism and the 6¼ inch equatorial the corona seemed to form pretty regular rings round the dark moon, of different heights according to the amount of light utilized by the instrument, on the photographic plates the corona, which, as I have before stated, exceeds the limits actually seen in the instrument I have named, has a very irregular, somewhat stellate outline, most marked breaks or rifts (*ignored by the spectroscope*), occurring near the sun's poles, a fact perhaps connected with the other fact that the most active and most brilliant prominences rarely occur there.

From the photographs in which the corona is depicted actinically we pass to the drawings in which it is depicted visually. I would first call attention to two drawings made by Mr. Holiday, who formed part of the expedition, and in whose eye every one who knows him will have every confidence.

First there is a drawing made at the commencement of the totality, and then a drawing made at the end. There is a wonderful difference between the drawings; the corona is in them very much more extensive than is represented actinically on our plates.

Here is another drawing, made by Captain Tupman, in which again we have something absolutely different from the photographs and from Mr. Holiday's sketches, inasmuch as we get an infinite number of dark lines extending down to the moon, and a greater extension than in the photographs, though in radial places the shape of the actinic corona and some of its details are shown.

Now the corona, as it appeared to me with the naked eye, was nothing but an assemblage of bright and dark lines, it lacked all the structure of the photographs and appeared larger; and I have asked myself whether these lines do not in some way depend on the size of the telescope, or the absence of a telescope. It seems as if observations of the corona with the naked eye, or with a telescope of small power, may give us such lines; but that when we use a telescope of large power, it will give, close to the moon, the structure to which I have referred, and abolish the exterior structure altogether, leaving a ring round the dark body of the moon such as Professor Respighi and myself saw in our prisms, and in the 6-inch telescope, in which the light was reduced by high magnification so as to bring the corona to a definite ring some 5 minutes high, while Professor Respighi, using a

4-inch telescope and less magnifying power, brought the corona down to a ring something like 7 minutes high.

And here we have an important connection between spectroscopic and telescopic work. If we employ a telescope in which the light is small or is reduced by high magnification, we bring the corona to a definite ring, and perhaps here we have the origin of the 'ring-formed' coronas.

Many instances of changing rays, like those seen by Plantamour in 1860, were recorded by observers in whom I have every confidence. One observer noted that the rays revolved and disappeared over the rifts.

We have next to deal with the polariscopic observations.

Mr. Lewis, in sweeping round the corona at a distance of 6′ or 7′ from the sun's limb, using a pair of compensating quartz wedges as an analyzer, which remained parallel to itself while the telescope swept round, observed the bands gradually change in intensity, then disappear, bands of a complementary character afterwards appearing, thereby indicating radial polarization.

Dr. Thomson at Bekul saw strong traces of atmospheric, but none of radial polarization, with a Savart. With the same class of instrument the result obtained by myself was precisely similar; while on turning in the Biquartz, at the top and bottom of the image of the corona, *i.e.* near the sun's equator, faint traces of radial polarization were perceptible for a short distance from the moon's limb. Captain Tupman, who observed with the polariscope after totality, announces strong radial polarization extending to a very considerable distance from the dark moon.

Leaving the extreme outside of the corona as a question to be determined at some future time—and it can well wait—let us come to the base of the corona, and deal with the region to which I have already referred, close to the sun.

What was the general conclusion at which we arrived on this important point? Before I state it, let me tell you the instrumental conditions of the inquiry. We can use such a spectroscope as the one with which you are all familiar, and so arrange matters that the slit shall be carried by a clock, so that it may follow accurately the edge of the moon; but if the least variation in the rate of motion takes place, the observation is rendered almost valueless. But if we employ a spectroscope, in which we sum up the light—do not localize the light, but throw it together—it does not matter whether your clock goes well or not, you are certain to have a result worthy of credit. But if you employ such an instrument as Professor Respighi employed, and abolish the slit altogether, the weight of any observations made with such conditions is very great.

Captain Maclear, who was observing with me at Bekul, has undoubtedly shown that when the light of our atmosphere is cut off by the interposition of the dark moon, we see very many more bright lines than we do when this is not the case, the lines being of unequal height.

Mr. Pringle, also at Bekul, showed that, at the end of totality,

many lines flashed into one of these instruments, carried under these difficult conditions.

Captain Fyers, the Surveyor-General of Ceylon, observing with a spectroscope of the second kind, saw something like a reversal of all the lines at the beginning, but nothing of the kind at the end.

Mr. Fergusson, observing with a similar instrument, saw reversal neither at the beginning nor the end.

Mr. Moseley, whose observations are of great weight, says that at the beginning of the eclipse he did not see this reversal of lines. Whether it was visible at the end he could not tell, because at the close the slit had travelled off the edge of the moon.

Professor Respighi, using no slit whatever, and being under the best conditions for seeing the reversal of the lines, certainly did not see it at the beginning, but he considers he saw it at the end, though about this he is doubtful.

From the foregoing general statement of the observations made on the eclipse of last year, it will be seen that knowledge has been very greatly advanced, and that most important data have been obtained to aid in the discussion of former observations. Further, many of the questions raised by the recent observations make it imperatively necessary that future eclipses should be carefully observed, as periodic changes in the corona may then possibly be found to occur. In these observations the instruments above described should be considered normal, and they should be added to as much as possible.

I had intended, if time had permitted me, to point out how much better we are prepared for the observation of an eclipse now than we were when we went to India, and how a system of photograph record should be introduced into the spectroscopic and polariscopic work; but time will not allow me to do more than suggest this interesting topic. I am anxious, however, that you should allow me one minute more to say how very grateful we feel for the assistance rendered by all we met, to which assistance so much of our success must be ascribed. I wish thus publicly to express the extreme gratitude of everyone of our Expedition to the authorities in India and in Ceylon for the assistance we received from them; and our sorrow that Admiral Cockburn, a warm and well-known friend to Science, who placed his flagship at the disposal of the expedition, and the Viceroy, whose influence in our favour was felt in every region of India whither our parties went, and to whom we gave up our ship, are now, alas! beyond the expression of our thanks. We are also anxious to express our obligations to the directors and officers of the Peninsular and Oriental Company for the magnificent way in which they aided us. If they had not assisted us as they did, Science would have gained very much less than she has done from the observations of the last eclipse.

[J. N. L.]

WEEKLY EVENING MEETING,

April 19, 1872.

Sir Frederick Pollock, Bart. M.A. Vice-President, in the Chair.

A. Vernon Harcourt, Esq. F.R.S. Sec. Chem. Soc.

On the Sulphurous Impurity in Coal Gas.

The luminous flames we use to light our houses after sunset are all equally gas flames.

The gas which burns round the wick of a candle is formed by the action of heat on the grease of which the candle is made. The gas which burns over the wick of an oil lamp is formed by the action of heat on the oil. The gas which burns over a "gas-burner" is formed by the action of heat on coal.

By the heating of any of these substances many different kinds of gas are produced, some of which become liquids if they are allowed to cool, while others are permanently gaseous. In the case of candles and lamps, when the hot gas is burnt as fast as it is made, the uncondensable and the condensable are burnt alike, the latter contributing even more than the former to the luminosity of the flame. In the case of coal gas, which is kept for many hours, and has often to travel along many miles of iron piping before it is burnt, only very little condensable gas reaches the burner. In this respect "gas" illumination is at a great disadvantage as compared with candles or lamps. To counterbalance this disadvantage gas must be produced from some substance which is much cheaper than grease or oil, and such a substance we have in coal. But coal, like all minerals obtained on the large scale, is mixed with small quantities of other substances, and, in particular, masses of coal always contain a greater or less proportion of a mineral known, according to its form, as pyrites or marcasite, composed of eight parts of sulphur united with seven of iron. When coal is heated, a part of the sulphur from this mineral unites with the carbon and hydrogen of the coal, and thus the illuminating gas formed from coal is contaminated with at least two sulphur compounds—carbon bisulphide and hydrogen sulphide. Such gas yields when it is burnt, in addition to water and carbon dioxide, sulphur dioxide produced by the burning of the sulphur. This gas has a well-known pungent smell; it acts on various colouring matters, and it is gradually changed in presence of air and moisture into a far more destructive substance called hydrogen sulphate or oil of vitriol. Unlike sulphur dioxide, which is a gas and can be removed by ventilation, hydrogen sulphate

is not volatile, and exercises a continued corrosive action upon organic materials or fabrics on which it is deposited. In a furnished room the leather bindings of books and coverings of furniture are especially liable to be injured thus; perhaps because this material, being a better conductor of heat than others which are used for the same purposes, is more bedewed after gas has been lighted in a room than they are, and also because it requires less often to be cleaned or renewed. Fortunately the conversion of sulphur dioxide into hydrogen sulphate takes place so slowly that in a room lighted with gas and well ventilated a very small part of the sulphur which gas is liable to contain remains in this destructive form. Fortunately, also, the quantity of sulphur which gas as at present manufactured is liable to contain is very small. In a room lighted with four gas-burners the volume of sulphur dioxide mixed with the atmosphere of the room in the course of an hour is about one-hundredth of a cubic foot. The amount actually present at any moment, if the room is fairly ventilated, is a small fraction of this volume. Nevertheless, it is possible, and even probable, that some injury, especially to the bindings of books, may in the course of years be caused under these circumstances; and it is desirable, with a view to avoiding any such injury, and also with a view to allaying the apprehension of it, that the proportion of sulphur compounds in coal gas should be reduced as far as possible.

A ton of coal may contain about 30 lbs. of sulphur: it yields nearly 10,000 cubic feet of gas, and a considerable part of the sulphur contained in it is given off with the gas in combination with either carbon or hydrogen. Of these two elements, hydrogen claims by far the larger proportion, not less than ten parts for one that is united with carbon. For the purification of gas from hydrogen sulphide excellent methods are everywhere in use. The gas is passed through layers of slaked lime or of iron oxide, by either of which substances all the hydrogen sulphide is capable of being completely absorbed. But the smaller quantity of sulphur existing in the form of carbon bisulphide is not arrested by these agents, nor is there at present any material or process known by which it can be effectually removed.

When coal gas which is pure from hydrogen sulphide is heated and tested again, it is found to contain this impurity, showing that some ingredients of the gas are capable of producing hydrogen sulphide by their mutual action. Hydrogen seems to have a much stronger affinity for sulphur than carbon has. One consequence of this difference is the unequal partition of the sulphur between the two elements in the gas retort. But this inequality does not reach its limit in the short time which elapses between the formation and cooling down of the gas; and accordingly, when foul gas (or gas which has not been purified from hydrogen sulphide) is further heated, the proportion of hydrogen sulphide in it is increased, and that of carbon bisulphide diminished. From a number of experiments in which foul gas was passed through an iron tube three inches in diameter, filled with iron turnings and heated for a length of about

two feet to low redness, it appeared that the amount of carbon bisulphide could be so far reduced that the gas, after purification from hydrogen sulphide, contained five or six instead of thirty grains of sulphur in 100 cubic feet. The gas was driven through the heated tube at a rate of from one to two cubic feet a minute.

A somewhat greater reduction in the amount of sulphur is obtained by heating the gas after, instead of before, purification and purifying it a second time. If it is the case, as seems probable, that the sulphur present in coal gas distributes itself when the gas is heated between the carbon and hydrogen in a ratio dependent upon the relative affinity for sulphur of the two elements, the proportion of carbon bisulphide to the total sulphur in the gas will be always the same when the composition of the gas is the same, and when it has been heated long enough for the establishment of an equilibrium. Accordingly we should expect the removal of sulphur, by the conversion of carbon bisulphide into hydrogen sulphide and the absorption of the latter, to be accomplished more effectually with gas from which the chief part of the sulphur had already been extracted. And this, as has already been stated, is found to be the case.

The nature of the chemical change which takes place when coal gas is heated may be illustrated by passing hydrogen over the mouth of a tube containing carbon bisulphide, and thence through a piece of combustion tubing heated nearly to a red heat. The mixture of hydrogen and carbon bisulphide vapour has no action on a solution of lead acetate; but, after the application of heat, the gas which issues produces at once a black precipitate, proving that hydrogen sulphide has been formed. This change occurs readily with hydrogen which has been carefully dried; but the presence of moisture appears to promote it; and as coal gas contains a quantity of aqueous vapour, much more than sufficient to react with the maximum amount of carbon bisulphide, it is possible that the formation of hydrogen sulphide when coal gas is heated, may be partly due to the intervention of moisture.

If clean iron nails are heated to redness in a glass tube, and coal gas is passed slowly over them, a soft black carbonaceous deposit is formed, and the gas is deprived of a part of its carbon. If, however, the gas be passed through more rapidly, no such deposition takes place, although the time of contact of the gas with the heated surface is still sufficient to effect the conversion of the carbon bisulphide into hydrogen sulphide. In the latter case it may be presumed that no change occurs in the illuminating power of the gas. But to establish a point which is of capital importance, some direct observations were made on the illuminating power of gas thus treated. It was found with gas passing at the rate of 5 cubic feet an hour through a half-inch iron tube, heated for a length of 12 inches, that, when the heat did not exceed low redness, no change was observable. When the heat was raised to bright redness, there was a perceptible increase in the illuminating power.

If the process of heating coal gas, in order to remove the sulphur contained in it, should be employed on the manufacturing scale, the rate of transmission of the gas through the heating apparatus would necessarily be such as to render any deposition of carbon very unlikely. But even where such deposition takes place, it is not necessarily accompanied by a diminution of the illuminating power.

An interesting experiment, from this point of view, is the decomposition of marsh gas by the electric spark. When a stream of sparks from a Ruhmkorff coil is transmitted between the ends of platinum wires through a small quantity of marsh gas enclosed in a glass tube over mercury, the gas gradually expands. In about ten minutes it is nearly doubled, and at the same time a black deposit appears on the tube, in the neighbourhood of the wires. Here the intense heat applied has effected an almost complete decomposition of the hydrocarbon into its elements. But at the same time there is found a small quantity of some more condensed hydrocarbon, probably acetylene. On expelling the gas through a jet attached to the upper end of the tube, and burning it, the flame is seen to be much more luminous than that of marsh gas itself. The fact of which this experiment gives a striking illustration is that the illuminating power of gas depends much more upon the nature of the hydrocarbons it contains, than upon the total amount of carbon. How great would be the gain to the manufacturers of coal gas, if such an operation as this were possible on the large scale, by which the volume of gas is doubled and its illuminating power, at the same time, greatly increased!

As far as chemistry is concerned, the simple operation of heating gas appears to offer the means of a sufficiently perfect purification. The construction of a suitable system of iron pipes for heating the gas, and the best mode of obtaining and applying heat, is a problem for the engineer. On the scale on which gas is manufactured, all the apparatus for dealing with it must be of a magnitude to which it is difficult to pass, even in imagination, from the small scale of laboratory experiments; but, otherwise, the problem does not appear to be one of any peculiar difficulty. It may perhaps be found possible to employ some of the waste heat of the retort-house for this purpose, and thus to effect the required purification without much increasing the consumption of fuel.

[A. V. H.]

WEEKLY EVENING MEETING,

Friday, April 26, 1872.

WILLIAM SPOTTISWOODE, Esq. LL.D. M.A. Treasurer R.S. and R.I. Vice-President, in the Chair.

JOHN STUART BLACKIE, F.R.S.E.
PROFESSOR OF GREEK IN THE UNIVERSITY OF EDINBURGH.

On the Philological Character and Genius of the Modern Greek Language.

THE Greek language, as the only living bridge between the intellect of the present and that of the great past, is certainly one of the most remarkable phenomena in the field of philology. To show how this wonderful language has retained its vitality, and now flourishes in a green old age, or rather in a lusty rejuvenescence, while Hebrew, Latin, Sanscrit, and all the great bearers of ancient culture, are numbered with the dead, is one object of the present discourse; and the other object is to take the language in the phasis ultimately assumed, and by analysis endeavour to set forth succinctly the chief characteristics of this most recent type.

In following the historical fates of this famous language, the fundamental principle with which we must start is that there is no such thing as absolute fixation in a living language. All language, so long as it lives, that is, so long as it is the organ of expression to a living people, and is not summed up to a bookish finality by lexicographers and commentators, must be subject to the change which is of the very essence of life. But though in a living language the vital force must constantly be at work, and will always be sending forth new shoots, as a vigorous tree sends forth new branches, this tendency to change is kept under the strongest check by those conservative forces universal in nature, which strive obstinately to keep up an existing type, when once established. These forces are of two kinds, internal and external; the internal are the powers of commanding intellect, of plastic genius, to which the masses of men instinctively concede an authority in matters of style and expression; the external are the two great institutions of Church and State, acting either from one common centre, or from two distinct, and in their own domain equally powerful, centres. The effect of such great centres of political and social action in setting a limit to the local

varieties of human speech, and stamping some favoured form of a language with a despotic authority, is too obvious to require illustration. Now with respect to both these classes of forces, the Greek language possesses a momentum, and springs of permanent energy, such as have fallen to the lot of none of the great ancient languages. A long succession of poets, philosophers, and scientific men in every field of intellectual expression, created and sustained for that form of human speech an intellectual dictatorship which could only be shaken by disturbing forces of the most violent kind. But when to this inherent imperial power of intellect there was added the unifying influence of a strong centralizing government at Byzantium, and the intense inspiration of a common Christian Church at once popular in its doctrine and aristocratic in its government, the combined conservative forces became so strong that the rankest undergrowth of linguistic mutation in the uneducated classes of society was unable to peer into the light, above the luxuriant leafage of the magnificent old forest of inherited classicality. That such an undergrowth did exist at Byzantium we have the amplest evidence in the two poems written in the popular dialect by the monk Theodorus Ptochoprodromus, about the middle of the twelfth century, and dedicated to the Emperor Manuel Comnenus. These poems are written in a dialect so corrupt, that it is extremely difficult, even with the help of the commentary of the learned Greek Adamantine Koraes,* in not a few places, to fish out their meaning. There existed, therefore, in Byzantium at the time of the Crusades, and when the Greek Empire was tottering surely, though very slowly and unwillingly, to its fall, a distinct and well-marked bistratification of the Greek tongue—the phenomenon of a classical type of speech inherited from the mighty past, used as the only legitimate form of expression by all men of education, and co-existent with this a vulgar type, the natural organ of the uneducated masses, and used by them in their colloquies, ballads, songs, and other light fly-leaves of purely popular expression. This bistratification continued with no doubt increasing force to mark the speech of the Greek people till the great shake which their nationality received from the taking of Constantinople by the Turks in 1453; and from this date it might naturally have been expected that, one of the strongest conservative bonds having been snapt asunder, the cohesive force that maintained the old classical type would cease, and a new language, standing to ancient Greek in the same relation that Italian does to Latin, would arise out of the *débris* of the crushed nationality of the conquered, mingled with the intruded elements of the foreign conquest. But this event did not take place; and why? first, because the weight of inherited intellectual and ecclesiastical authority was far higher in the case of Greek than in that of Latin; and again, because the character of the Ottoman policy and religion was such as to render a fusion between the language of the conquered

* In the first volume of his Ἄτακτα.

and the conquerors—such as took place in Italy to a certain extent, and in England to a much larger extent—impossible. The Greeks hated the Turk with an intense hatred, even when they condescended, as in the case of the Fanariotes, to serve him, and to make profit of him; so that the smothered embers of patriotism, of which the language was a part, were fanned into a flame by the very forces that under different circumstances would have extinguished them. The Greek language therefore, in spite of that terrible dilapidation and prostration of the old social fabric, remained Greek, and no new language, as in the nations of Western Europe, was created; and it continued for four centuries, till the great revolt in 1821, sprinkled, no doubt, or spotted over in various motley phases with barbarisms and adulterations, presenting characteristic local varieties of a debased type, very offensive to the nice nose of academical men, but retaining all the while a stout muscular heart pumping with genuine Hellenic vitality, and able at any moment to throw off the scurf and blotches of a merely external affection, when purer air and a more nourishing diet should have produced a healthy condition of the popular blood. It was in this state that the Greek language was found by the great patriot and scholar whom we have already named, Adamantine Koraes (*nat.* 1748), who, in a long series of learned works, effectually established himself as an authority in the constitution of the fixed type which the Greek tongue, after so many centuries of unkindly handling, was ultimately destined to assume. Koraes distinctly saw that, if the Greek language was to be used by Greek men of letters, for the purpose of improving and elevating the Greek people, it could only be in a genuine popular form, that is to say, by shaking off altogether the polished affectations of the inherited classical speech, and addressing the living Greek people in that living form of speech which they had sucked in with their mothers' milk. While doing this, however, it was by no means necessary that this noble friend of his oppressed people should adopt every peculiarity of the popular dialect that was manifestly the result of blind ignorance or random carelessness. With a nice discriminative perspicacity he seized the grand features of the Romaic idiom, while he brushed away or washed off its superficial defacements. In fact, with the wisdom of a practical man, he made a compromise between the utterly chaotic corruptions of the lowest ballads of Cretan or Epirotic Greek, and the style of bookish rather than of popular origin, to which the learned Greeks naturally inclined; and this compromise —the result not of Koraes' literary position solely, but of a general under-current of national feeling—produced what is now called the Neo-Hellenic dialect, to distinguish it from the Romaic, or lowest popular stratum of Greek, inherited from the uneducated masses of the Byzantine Empire.

In now attempting a succinct philological analysis of this type of Greek speech, we must bear in mind that, from the very nature of the case, there is no such fixed standard of modern Greek as there was of

classical Greek in the days of Attic supremacy. For the existing Neo-Hellenic, the best standard, no doubt, may be assumed to be the Greek newspapers; but from the strong tendency to purification and restoration which has manifested itself among all Greek writers since the great popular up-rising of 1821, it is better for philological analysis to take the current Greek of the last century, affected to a certain extent by Italian influences, as a just mean between the extreme corruption of the Cretan dialect, and the Greek of the modern Greek newspapers, which present, according to the taste of their writers, in not a few points, a greater or less approximation to the ancient classical style. To tune my ear to this genuine Romaic as contrasted with a restored semi-classical Greek, I have been in the habit of using a translation of the 'Arabian Nights,' published at Venice in 1792; but I have also carefully read through the chivalrous poem of the 'Erotocritus,' composed by Vincentius Cornaro, a Cretan Greek, about the beginning of the last century; as also the collections of popular Greek ballads by Passow, Kind, and others, the peculiarities of which, however disowned in the written style of educated Greeks, could not be overlooked by the philologer. It is proper to state also that, though the propositions which I lay down are drawn out directly from a pretty wide field of Romaic reading, I was both confirmed in my conclusions, and greatly benefited in my researches, by the excellent works of those few others who have worked laboriously in this neglected corner of the Hellenic vineyard. The names of Sophocles, Mullach, Passow, Ellisen, Pashley, Tozer, Geldart, Clyde, Le Grand, d'Eichthal, and the late Viscount Strangford, will sufficiently indicate to the intelligent the sources from which I have derived the greatest amount of assistance.

The first remark that we feel called on to make, in sketching the physiognomy of modern Greek, is by way of protest against the vulgar notion that all the changes which take place in a language deviating from an inherited classical type, are to be considered as corruptions, or varieties of phonetic decay. No doubt both Italian and modern Greek, when contrasted with classical Latin and Attic Greek, contain not a little of what, with strict scientific precision, may fall under the head of corruption; but the mistake lies in the easy presumption that this category covers the whole facts of the case. The truth rather is that, while by the slackness of the controlling power of the conservative forces, any language may be encrusted with corruption or have its finest features rubbed off by phonetic decay, it may at the same time, through the force of its vigorous vitality, go on sending forth new blossoms, and revelling in fresh formations which are in no sense corruptions, but only the legitimate outgrowth, foliation, and efflorescence of an essentially vital thing. The test of the true character of such new formations, to the eye of the philologer, is not the artificial authority of some fashionable Cicero or other great master of style, but simply the conformity or non-conformity of the recent growth with the old type of which it purports to be a part. There-

fore, such words in Latin, for instance, as *gratitudo* and *beatitudo*, are not corruptions or defacements, but expansions and improvements of the Roman tongue, though a famous ancient orator never did use them, and a famous modern Cardinal never dared to use them. In the same way Greek never resigned and could not resign, in favour of an artificially constituted authority of a great book-maker, or a society of book-makers, its inborn right of luxuriating in all sorts of new formations; and, if Smyrnean Homer could say γλυκερός as well as γλυκύς, the Byzantine Greek of later days will insist on his right to say βρωμερός as well as βρωμώδης, and τάχινος as well as ταχύς. And in verbal affixes also, if the ancients could say, not only ὁμαλόω, but ὁμαλύνω and ὁμαλίζω to express the one idea *equalize*, the moderns will not be scouted because they prefer δακρύζω to δακρύω, ἀρχίζω to ἄρχομαι, πασχίζω to πάσχω, and other formations of the same kind. And not only will they claim, as the genuine inheritors of Homeric and Aristophanic speech, the right to make new terminations to old verbs, but they will make new verbs from old roots, and luxuriate in a wealth of new compounds, to which the only legitimate objection shall be that they are formed on a false analogy, not that they are either superfluous or unauthorized. For in shaping forth their own forms of expression the people require no authority, but the consistent truthfulness of their own linguistic instinct; and, as to superfluity, Nature everywhere rejoices in it, and, though she may have reasons for tolerating the pruning hook, never submits to systematic castration. I have, from my reading, made some curious collections of the riches and variety of mediæval and modern Greek in this reign of exuberant new phraseology; but I can do no more than simply allude to them here, and refer the inquirer shortly to Kind's little Romaic dictionary, or any work of the same kind, where he can scarcely run his eye down a column without encountering some new word of rare poetic power and beauty, formed according to the truest analogy of genuine Hellenic speech. On the other hand I have not the slightest wish to conceal the fact that there are certain changes marking the physiognomy of modern Greek which fall strictly under the category of corruption, and which to the eye of the philologer appear just as false as an ornament in architecture, not organically growing out of the construction, as all true ornament does. Such, for example, are the terminations ίλλα, ίζα, and οὐστικος, in diminutives, and the termination άτος in adjectives, as in γεμάτος from γέμω, borrowed from the Latin, and with which the verses of Ptochoprodromus are plentifully interlarded.

The first and most universal form of corruption to which all languages are subject, is CURTAILMENT. From motives of indolence, haste, or convenience, polysyllabic words are likely to be shortened in various ways; sometimes amputated in the front, as when the American says *'cute* for *acute;* sometimes at the end, as when an Italian says *amor* for *amore;* sometimes by retaining the initial and final consonants, and crumpling up the middle, as when the German

says *Pfingst* for *Pentecost*. Of all these methods of curtailment we have copious examples in modern Greek. Of initial amputation, or what the grammarians call *aphæresis*, λίγος for ὀλίγος, πίσω for ὀπίσω, πωρικά for ὀπωρικά, may serve as specimens; they include a large class, practised generally by the uneducated, though from persons who have any pretensions to culture, at this time of day they may be scarcely acknowledged. Of the final amputations, or what the grammarians call *apocope*, by far the most striking are the cutting off of the ν from the termination of nouns, and of the ον from that of diminutives. Thus for καλόν we have καλό, and for καλόν παιδίον, καλό παιδί. Words thus amputated do not offend the ear, and their true character is generally not difficult to recognize. But when both head and tail are cut off, as sometimes happens, then only the practised philologer will easily recognize to what animal the remanent trunk belongs. Ψάρι, a *fish*; μάτι, an *eye*; λάδι, *oil*; ξύδι, *vinegar*; and σπῆτι, a *house*, are all old classical words amputated before and behind; but only the scholar, by restoring their perfect forms, could spell out their signification—ὀψάριον, ὀμμάτιον, ἐλάδιον, ὀξιδύον, *hospitium*. But this double amputation, how strange soever it may appear to us in modern Greek, is one of the common processes according to which German has passed into English. Thus English, *fought*; old English, *foughten*; German, *gefochten*. In the Greek, this same tendency to curtailment which induced us to reject the *ge* of the past participle, led the Epirotic ballad makers to say γραμμένος for γεγραμμένος, and δάσκαλος for διδάσκαλος. A very peculiar form of curtailment appears in Romaic, when the governing preposition coalesces with the definite article, as where σταν πόλιν stands for ἐς τὰν πόλιν, which the Franks, who heard it with the ear, without the understanding, corrupted into the well known *Stamboul*. In the same way ἐς τὰν Κῶ became *Stanco*. In like manner the Italians say *pel* for *per lo*, and *nel* for *in ello*. Of syncope, or curtailment by the retention of the final consonants, with the slurring of the intermediate letters, we have the most notable examples in λεν for λέγουν (*i. e.* λέγοντι, Doric for λέγουσι), πᾶν for πάγουν, φᾶν for φάγουν, and a few others of the same kind. Of *crasis*, which is akin to syncope, and technically denotes the form of curtailment so common in Sanscrit, which takes place when the final vowel of the preceding word flows into and forms a new vocalic compound with the initial vowel of the immediately following word, we have in Romaic such examples as νάλθη, or νάρθη, for ἵνα ἔλθῃ, τώχει for τὸ ἔχει, πώρχομαι for που ἔρχομαι, τῶπα for τὸ εἶπα, and a few more. But we must by no means allow ourselves to suppose that because curtailments and amputations of various kinds are frequent in modern Greek, this dialect of the Hellenic is therefore specially marked by a meagre and monosyllabic character. On the contrary, the tendency to polysyllabic structure and composition is so strong in Greek that it recreates in one direction what it has destroyed in another; besides that the amputated forms of polysyllabic classical words are very seldom monosyllables, as when in English the German

haben becomes *have*, but dissyllabic or trisyllabic. Nay, in one of the commonest species of amputation the modern form has always as many syllables, sometimes more than the classical. This arises from the fact that the modern amputation takes place not on the original classical word, but on its diminutive, systematically used for the primary noun. Thus κεφαλή produces κεφάλιον, from which the modern κεφάλι, ποταμος ποτάμιον, whence ποτάμι and many such. And thus it has come to pass that, in spite of all amputations, in point of sonorous syllabic amplitude the modern dialect is not one whit inferior to its classical prototype. So much for corruption by curtailment. But the breaking down of an old type of speech is not characterized merely in this negative fashion by the absence of something which ought to have been present, and whose presence may be lightly spared. There are positive and protrusive corruptions in the form of additions, on which we may now cast a glance. When we talk of additions, however, we do not always necessarily mean that the increased form of the word is of comparatively modern date; on the contrary, the fuller form may in certain cases be one of the oldest in the language, brought to the surface in the modern dialect, from the displacement of the dominant Attic. The true character of such increments, whether in the shape of prepositive, paragogic, or epenthetic letters or syllables, may often defy the combined erudition and sagacity of a Lobeck to determine;* it will be enough for our present purpose simply to indicate their character, without attempting to investigate their origin. Sometimes, as in Italian *sono* from *sunt*, a vowel seems added to the third person plural of the verbal termination; as in Doric, λέγοντι; Latin, *legunt;* Romaic, λέγουν, and also λέγουνε; in the same way ἐλέγανε for ἔλεγαν. Among the pronouns we find the lengthened forms ἐμένα and ἐσένα, τετοῖος for τοῖος, and ἐτοῦτο for τοῦτο. An epenthetic γ between two vowel sounds is very common, as in καίγω for καίω; σαλεύγω for σαλεύω; αὔγον for ᾠόν, in which some have been fond to recognize the digamma; but in many cases this soft gamma (like the g in the German *Göttingen*) is plainly only a variation of the *spiritus asper*, as in the first syllable of γυιός for ὑιός, γιατρεύω for ἰατρέυω, &c. One of the most notable of epenthetic letters is the ν inserted before the personal suffix of certain pure and liquid verbs, as in φέρνω for φέρω, στέλνω for στέλλω, σκοτώνω for σκοτόω; a peculiarity which appears already in the Alexandrian Greek of the Septuagint; and indeed, in some sporadic instances, is as old as Homer, who uses δύνω for δύω, and θύνω for θύω.

Along with curtailment and increment, and certainly not of secondary importance in the reconstruction of all languages in a state of disintegration, nascent or complete, is the tendency to simplification of flexional forms, and regularization of traditional anomalies. By the influence of this tendency, the verbs in μι have been altogether

* See the 'Pathologia Græci Sermonis,' Königsberg, 1853, where this subject, in reference to ancient Greek, is handled in the author's exhaustive and masterly style.

abolished. For δίδωμι we have δίδω, for τίθημι, θετῶ; while for the irregular aorists ἔδωκα and ἔθηκα, appear δωσα and ἔθεσα. Among the declensions there is a manifest favour, for the first in ας and α; which leads, in a large class of cases to the ignoring of the third declension altogether, and the conversion of its accusative case—after the analogy of the Italian—into a new nominative of the first declension. Thus πατέρα is assumed as the base of a new nominative πατέρας declined after the norm of the first declension, and πεδιάδα the accusative of πεδιάς, takes the place of πεδίον as a feminine of the first declension. By the same instinct, the irregular comparative and superlative forms of καλός, πολύς and μέγας have disappeared, and καλήτερος, πολλότατος, and μεγαλώτατος usurp their place.

So much for variations in the form of words taken singly; but there are changes of no less importance in the manner of combining words together to form a sentence—what we call syntax. Now, as the moods and tenses of the verbs are the principal engines by which the dependency of one clause on another is expressed, it is manifest that changes in the syntax of such a finely articulated language as the Greek must imply changes in the verb; and these changes appear principally in the form of loss or curtailment already mentioned. The two principal losses which the verb has suffered, producing striking changes in the syntax, are those of the optative and the infinitive mood. The loss of the optative, which in the New Testament has already been curtailed of the largest part of its original domain, leads to the habitual violation of the law so strictly observed by the Roman writers, called *consecutio temporum*, that is to say, the law by which—to illustrate the matter from our own usage, a dependent clause with *might*, *could*, *would*, or *should*, the equivalents of the Greek optative, must regularly follow a principal clause, with the leading verbs in the past tense, which sequence in fact is only the preservation of a natural and very obvious congruity. If, instead of observing this plain rule, the modern Greek uses a form of speech which, translated into English, would sound thus—I ADVISED *you to do so, that you* MAY *be aware of the difficulties of the case*, he certainly has sacrificed the perfect logical consistency of his syntax, but happily without the least harm either to the intelligibility or the euphony of his sentence. Another scarcely less awkward loss which the verb has suffered, is the entire disappearance of the infinitive mood, for which, after the Latin fashion, the subjunctive mood with the conjunction να for ἵνα is substituted. This is simply the making universal of the common English formula, occurring in such a sentence as *It is too bad* THAT YOU SHOULD DO *so;* where in classical Greek the accusative before the infinitive was the only tolerated construction. Another very serious loss to the verb is that not only of the pluperfect tense—very little used by the Attic writers themselves—but also of the future, and the compensation for both losses by the modern device of auxiliary verbs. No less significant of declension from the ancient classical syntax is the total absence of the dative case of

nouns, with the substitution for it of the genitive, or more generally the accusative, governed by all sorts of prepositions. All these losses put together, no doubt give a certain modern cast to Neo-Hellenic sentences, which the classical scholar naturally resents; conjoined with these, the substitution of ὅπου for ὅς as a relative, and the invention of some dozen new conjunctions, are small matters; but it is wonderful how soon the ear becomes reconciled to changes of this kind; and it is no less surprising with what tact and dexterity accomplished writers now handle the language so as to reduce to a minimum the offence which these modernisms are apt to occasion. Indeed, there seems little reason to doubt, that, if the education of the Greek people shall proceed for another generation in the same direction, and at the same rate which have marked it since the great liberation, the popular speech will have reverted to its old classical forms, and the dative case, with the infinitive mood and the future, all have become as familiar to the tongue of the common man, as they are now to the ear of the well-disciplined scholar.

Not the least important element, certainly, in any estimate which may be made of the æsthetical character of the Neo-Hellenic dialect, is the matter of PRONUNCIATION, including the two principal points of vocalization and accentuation. Now with regard to vocalization, in which the melody of language principally consists, we must disabuse ourselves at starting from the very natural conceit that our English method of enunciating the Hellenic vowels and diphthongs has the slightest claim either to scientific authority or to æsthetical beauty; it is a pure figment, an arbitrary local usage, having as little that is truly Greek in its character as French or Italian would have, if pronounced by a stupid or careless transference to those languages of the peculiar laws of English orthoepy. I have no time to discuss this matter in detail here;* it must be sufficient to remark that the inversion of the natural scale of the vowels, which appears in the pronunciation of the English *a* and *i*, and the coarse canine sound given to the diphthong *ou*, when applied to Greek, cannot but produce a most disagreeable effect on a well-trained Hellenic ear. But even worse than this, and what makes English Greek, however fluent (which it seldom is), to the ear of a native altogether unintelligible, is the gross travestie of the words systematically perpetrated by the substitution of the Latin for the Greek accents, all but universal in the learned schools of Great Britain.† In opposition to this utterly lawless and arbitrary fashion of pronouncing one of the finest languages in the world, the living Greeks, whatever losses their rich and delicate orthoepy may have suffered in the course of long centuries, has certainly continued in the main true to itself; and its worst fault, the well-known Itacism, that is, the predominance given to the slender sound of the vowel ι in the utterance of some half-dozen vowels and

* See my essay on Greek pronunciations. Edinburgh, 1852.

† See this subject, fully treated in my paper on "The Place and Power of Accent in Language," in the 'Transactions of the Royal Society, Edinburgh,' for 1870–1.

diphthongs that could not originally have been so confounded, is but the culmination of a certain "gracilitas" which Quintilian noted in his day as one of the most distinctive peculiarities of classical Greek. As for the accents, thanks to Aristophanes of Byzantium, the inventor of the familiar accentual marks, and the service of a patriotic church, the popular ear has wandered from the old path in this matter, only in a very few and comparatively unimportant instances. In the general case the modern accent remains faithful to ancient usage; and the fact, which some learned men persist in doubting, that the Greek accent, like the Latin, actually did mean, not only elevation of tone, but increase of stress or emphasis on the accented syllable, stands out everywhere with unmistakable distinctness in the great army of words curtailed of their initial syllable to which we above (p. 498) directed special attention. For in these words, in every case it is the unaccented syllable, as in δὲν for οὐδέν, which falls off, and not the contrary.

How far the euphony of the language has been affected, or, perhaps, turned into a cacophony by any of the habits of modern Greek orthoepy, is to be determined, not by picking out individual words seamed with a special scar, as is sometimes done, but by the general impression which the language makes on the ear when used by those who use it for the purpose of producing an æsthetical effect. That *vasilefs*, for instance, the Neo-Hellenic utterance of *βασιλέυς*, is a less melodious word than the same word pronounced in the customary English fashion may be quite true; in all languages, and not least in English, there are single words which, when pronounced most rightly, are not pronounced most beautifully; but the general æsthetic effect of a language is not sensibly affected by such occasional cacophonies. As to a certain feebleness of vocalization supposed to be produced by the Itacism, it ought to be remembered that the instinct of the popular ear, like the instinct which guides the poet, will lead the people to avoid the near repetition of identical sounds, and there may thus in actual practice be no greater danger of satiating the ear with a surcharge of the slender ι, than in English we feel, when we have occasion to use such words as *facility*, *docility*, *agility*, *inanition*, *Elysian*, *precision*, and such like. As a matter of fact, I can state that in going through the Erotocritos, a poem nearly as long as the Odyssey, and the Klephtic ballads in Passow's collection, my ear was never disagreeably struck with any predominance of the sound *ee*. On the contrary, the broad, full, and peculiarly Greek sounds of *a* and *oo*, (English *oo*), appeared to me to preponderate. Feebleness certainly, or tenuity of sound, is the last charge that can be brought against such lines as

Ὀλημεροῦλα πολεμᾷ, τὸ βράδυ Καραοῦλι.

or

Σκοτώνει τοὺς Ἀγαρηνοὺς, πεζοῦρα καὶ καβάλη.

or

Πολλὴ μαυρίλλη ἔρχεται, μαυρὴ σὰν καλιακοῦδι.

from Passow, or

Σὰν, ὅταν μεσοπέλαγα δυὸ ἄνεμοι σηκωθοῦσι
Ἐξάφνου, καὶ μὲ τὴν βροντὴν φυσῶντας πολεμοῦσι,
Μάχονται μὲ τὴν θάλασσαν, μανίζουν καὶ φουσκώνουν,
Ταῖς ψιχαλίδες τοῦ γιαλοῦ στα νέφαλα σηκώνουν;
Ἕνας φυσᾷ ἀπ' Ἀνατολὴ κὶ ἄλλος ἀπὸ τὴν Δύσι
Πάσχει ὁ βορρᾶς, καὶ μάχεται τὸν Νότον νὰ νικήσῃ.

from Cornaro; and if this alleged offence of the Itacism be practically of so little significance, it is needless to remark that the other prominent peculiarity of modern Greek æsthetical composition, *viz.* the substitution of accentual for quantitative rhythm, does not in the slightest degree mar the euphony of Romaic verse. The same substitution, as everyone knows, took place both in Italian and in the hymnology of the Latin Church; and in both cases not to the damage of harmony, but with the creation of a new harmony, which may justly take its place as the legitimate sister of the old.

I have said nothing hitherto of that element of adulteration from foreign sources which generally appears when an old-established language, like Latin or Greek, begins to be shaken out of its original compactness. In Italian and French the Teutonic element occurs not scantily sprinkled through the mass of the old Latin; and in modern Greek, likewise, every foreign influence which lasted long enough for the purpose, was sure to leave its visible print on the face of the spoken dialect. So Byzantine Greek came to be spotted with Latin, the Greek of the mediæval Turkish kingdoms with French and Italian, and the Greek of the Epirotic Klepths with Turkish and Albanian. But the peculiarity of the Greek as contrasted with the French and Italian was and is that the foreign elements, by which it was at different times corrupted, were only attached externally to the original organism, not worked into the blood and bone; so that, like the scurf of a cutaneous disease, when the national health revived, they were naturally cast off, and this so completely, that at the present day the occurrence of a foreign word in a Greek sentence is as rare as the sight of a Turk in the streets of a Greek city.

I may conclude the present hasty sketch of philological features with two speculative remarks, the one retrospective, the other prospective. Casting the eye backward into the earliest history of Hellenic speech, the question may well be asked how far the present form of the language is to be looked on as the revival of the old Aeolo-Doric dialect, which had so long been suppressed by the graceful tyranny of the Attic; and to this question one answer lies very much on the surface, *viz.* that, as from the very nature of the case a certain sprinkling of non-Attic peculiarities was to be expected, and specially of the widely-dominant Doric, so, in point of fact, such a sprinkling does undeniably discover itself. The favourite ας of the masculine agent in the first declension is certainly Doric. Doric also is ζητᾷ for ζητεῖ, τηρᾷ for τηρεῖ; and not less τάζω for τάσσω, and other verbs of the same flexion. On the other hand, the favourite change of ο

and ω into οο as in κουδοῦνι for κώδων is Ionic; and more distinctly the slender η instead of α in the nominative singular feminine of adjectives in ρος as in μαυρή for μαυρά. To me, upon the whole, judging by the impression on the ear, the Doric seems the more potent of the two elements; it is, however, scarcely so marked as to justify the special designation of Aeolo-Doric, by which some writers have wished to baptize the present form of the spoken dialect.

The prospective question which a conjectural glance into the future suggests, is simply this, how far is it probable that the Greek language in its present state will survive any changes in the political system of Europe which the events of the next centuries are likely to produce? To this question the answer seems plain. There is no likelihood that a language which has survived so many changes of political decrepitude at home, and political violence from abroad, will be perniciously affected by any changes likely to take place in the balance of the European system. As long as the Greek kingdom, the creation of three strong and jealous powers, shall stand, even with the meagre amount of success of which hitherto it has been able to boast, so long, as a matter of course, must the Greek language in its present Avatar of restored beauty, go on prospering and to prosper. Should the little kingdom, on the other hand, be snuffed out some day, and become a part of one of the great empires that came forward to nurse its cradle, most probably of Russia, in this case also there is not the slightest reason to fear that a cultivated language, so rich in powerful intellectual tradition as the Greek, should give way to Slavonic in the same way that Gaelic, in the Scottish Highlands, is now giving way to the Saxo-Latin speech of the men of the Low country. Russia, besides, could have no motive that should induce her to wish to stamp out Greek in Athens, in the same way that she is endeavouring to stamp out Polish in Warsaw. The relation betwixt Greece as an incorporated part of Russia and the great Muscovite Empire, would rather be one of the most friendly description; and the Czar would be forced to preserve the language of the Greek Church and the Greek intellect, as one of the brightest gems in his crown. The Russian clergy, in fact, would be more ready to be Hellenized by studying Chrysostom and St. Basil, than the Greek people would be to sink their aristocratic nationality in a Slavism of huge geographical amplitude, but without intellectual power or literary prestige. And in this way the same captive Greece that conquered Rome two thousand years ago, may before the lapse of another century have conquered Petersburg and Moscow; and then the day may not be far distant when, to realize the bright imagination of an illustrious French Hellenist,* the formal and meagre Latin shall give place to the flexible and luxuriant Hellenic, as the Catholic organ of intellectual intercourse between educated men of all nations, from the Ganges to St. Francisco.

[J. S. B.]

* Monsieur Gustav d'Eichthal.

ANNUAL MEETING,

Wednesday, May 1, 1872.

SIR HENRY HOLLAND, Bart. M.D. D.C.L F.R.S. President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1871 was read and adopted.

Sixty-six new members were elected in 1871.

Sixty-three Lectures and Twenty Evening Discourses were delivered during the year 1871.

The Books and Pamphlets presented in 1871 amounted to 115 volumes, making, with those purchased by the Managers, a total of 217 volumes added to the Library in the year, exclusive of periodicals.

Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers and Visitors, and to the Professors, for their services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:—

PRESIDENT—Sir Henry Holland, Bart. M.D. D.C.L. F.R.S.
TREASURER—William Spottiswoode, Esq. LL.D. M.A. F.R.S.
SECRETARY—Henry Bence Jones, M.A. M.D. D.C.L. F.R.S.

MANAGERS.

George Berkley, Esq. C.E.
William Bowman, Esq. F.R.C.S. F.R.S.
George Busk, Esq. F.R.C.S. F.R.S.
John Peter Gassiot, Esq. D.C.L. F.R.S.
Sir John Lubbock, Bart. M.P. V.P.R.S.
George Macilwain, Esq. F.R.C.S.
John Carrick Moore, Esq. M.A. F.R.S. F.G.S.
William Pole, Esq. M.A. F.R.S.
Sir W. Frederick Pollock, Bart. M.A.
The Earl of Rosse, B.A. D.C.L. V.P.R.S.
Robert P. Roupell, Esq. M.A. Q.C.
Gen. Sir Edward Sabine, R.A. K.C.B. D.C.L. V.P.R.S.
The Marquis of Salisbury, M.A. F.R.S. Chanc. Univ. Ox.
Charles William Siemens, Esq. D.C.L. F.R.S.
Sir Charles Wheatstone, D.C.L. F.R.S.

VISITORS.

John Charles Burgoyne, Esq.
George Frederick Chambers, Esq. F.R.A.S.
Erasmus Darwin, Esq.
Edward Frankland, Esq. D.C.L. F.R.S. Pres. Chem. S.
Thomas Williams Helps, Esq. M.A.
Thomas Hyde Hills, Esq.
Thomas Lee, Esq.
Robert Pilkington Linton, Esq. F.R.C.S.
William Millar, Esq.
St. George Mivart, Esq. F.R.S. F.L.S.
Major-General Richard Cornwallis Moore, R.A. C.B.
Edward Henry Moscrop, Esq.
The Lord Josceline W. Percy.
Basil Woodd Smith, Esq.
Michael Wills, Esq.

WEEKLY EVENING MEETING,

Friday, May 3, 1872.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

William Spottiswoode, Esq. LL.D. M.A. Treasurer R.S. and R.I.

On Optical Phænomena produced by Crystals submitted to Circularly Polarized Light.

On a former occasion I exhibited some phænomena depending upon circular, or, as it was then also called, successive, polarization, and in particular I adopted and explained a method for producing circularly polarized light devised by Sir Charles Wheatstone. I propose on the present occasion to pursue the subject into some of its ulterior consequences. In terms of the wave theory, light is said to be circularly polarized when the vibrations are circular, as distinguished from plane polarization, when they are rectilinear. And further, it is known from mechanical principles that a circular vibration may always be produced by the combination of two rectilinear vibrations, the amplitudes or extents of which are equal, and whereof one is in advance or in rear of the other by one or by any odd number of quarter-wave lengths. In the former of these cases the circular motion will take place in one direction, say right-handed; in the latter in the opposite, say left-handed. The contrivance used for producing circular polarization this evening is known by the name of a "quarter-undulation plate," and consists of a plate of mica split to such a thickness that one of the two rays into which plane polarized light is divided on entering it is retarded by an odd number of quarter-wave lengths behind the other.

The optical phænomena produced by crystals when submitted to polarized light are usually divided into two classes, *viz.* (1) those arising from the use of parallel light, and consisting of broad sheets of colour; and (2) those due to convergent light, and consisting of the rings and brushes, the general character of which is well known. I propose to take a few specimens from each class, and to examine the modifications which the known phænomena undergo when the light is both polarized and analyzed circularly, *i.e.* when one quarter-undulation plate is interposed between the polarizer (Nicol's prism) and the crystal to be examined, and the second between the crystal and the analyzer (Nicol's prism).

In the first place, it is known that if a plate of selenite be placed in an ordinary apparatus when the polarizer and analyzer are either parallel or crossed, there are four positions at 90° apart in which the plate will produce colour; and further, that if the analyzer be turned through 90° the same result will be obtained, except that the colour will be complementary to that first seen. The intensity of the light at any given point is then given by the formula:

$$\cos.^2 s - \sin. 2i \sin. 2(i - s) \sin.^2 \frac{\theta}{2},$$

where i and s are the angles made with the original plane of polarization by the principal sections of the crystal and of the analyzer respectively, and θ is the retardation.

If, however, the two quarter-undulation plates (say the plates A and B) be introduced, the light undergoes the following processes:— First, it is plane polarized by the polarizer; secondly, the plate A being placed so that its axis is inclined at $\pm 45°$ to the original plane of polarization, the light undergoes right or left handed circular polarization, and in that condition falls upon the crystal; thirdly, in their passage through the crystal C the rays are each divided into two, whose vibrations are at right angles to one another, and whereof one is retarded in proportion to the thickness of C; fourthly, the plate B being placed so that its axis is parallel or perpendicular to that of A, each of these sets of rays is circularly polarized, one set right-handed and the other left-handed; fifthly, these two oppositely circularly polarized sets of rays combine, according to known mechanical laws, on emerging from B into plane rays, in which the planes of polarization of the different colours of the spectrum are turned through different angles. Hence finally by turning the analyzer round we shall cross these various planes in turn and successively extinguish the different colours, leaving the complementary colours visible. The system of plates A C B consequently acts in this respect like quartz. It is, however, to be observed that if the plate B be turned from one of the two proposed positions to the other, the directions of motion in the two emergent circularly polarized rays, and consequently the planes of polarization of the different colours, will be reversed; in other words, with the plate B in one position we shall imitate a right-handed, with the plate B in the other a left-handed, quartz. The intensity of the light at any point is then given by the formula:

$$\sin.^2 \frac{\theta}{2} \text{ for one position,}$$

$$\cos.^2 \frac{\theta}{2} \text{ for the other.}$$

Again if, the plates A B retaining either of the positions before indicated, the crystal C be turned round in its own plane; then, since the light emerging from A and B is circularly polarized, it has lost

all trace of direction with reference to the positions of the polarizer and analyzer, and consequently no change of tint will be observed. The same is abundantly clear from the formula written above, because the only term it contains depends upon the retardation within the crystal C. This experiment was made by Airy.

If the plates A and B have their axes directed 45° on either side of the axis of C, and the three plates be turned round as one piece, the colour will remain unchanged; while, if the analyzer be turned, we have the colours shown in the regular order. If the plates A and B have their axes directed at 45° on the same side of the axis of C, and the pieces be turned round bodily as before the colours change in the same order as above, and go through their cycle once in every 90° of rotation; and if the analyzer be turned in the same direction, the colours change, but in the reverse order. The explanation of this is to be found in the fact that when the plates A and B are crossed, the retardation due by A is compensated by that due to B; so that the only effective retardation is that due to the crystal C. But upon this depends the rotation of the plane of polarization; if therefore the polarizer and analyzer remain fixed, the colour will remain unaltered. When the plates A and B have their axes parallel, there is no compensation, and the colour will consequently change. This experiment was made by Fresnel. The mathematical expressions for the intensity of the light in the two cases respectively are:

$$\cos^2\left(j+i+\frac{\theta}{2}\right), \text{ and } \cos^2\left(j-i-\frac{\theta}{2}\right),$$

where i is the angle made by the principal sections of A with that of the polarizer, and j that of the principal section of B with that of the analyzer. The first expression is obviously unchanged when the angle between the polarizer and analyzer, *viz.* $\frac{\pi}{2}+i+j$, is unchanged.

It should be added that the rotation of the plane of polarization, and consequently also the sequence of tints, does not follow exactly the same law in the above cases as in quartz.

We now come to the case of convergent light, that is, to the phænomena of crystal rings. And let us examine the effects produced by the same arrangement as before, *viz.* two quarter-undulation plates A, B, one in front and one behind the crystal C. To quote from Mr. Airy:—"The first thing that strikes us in this combination is that there is nothing, except in the crystal, that has any respect to sides. For the only incident light is circularly polarized; the only light allowed to emerge is circularly polarized. The appearance therefore of the coloured rings, &c., must be such as conveys no trace of any plane of polarization, and must not vary as the crystal is turned round. In the common exhibition of the coloured rings the principal trace of the planes of polarization is in the uncoloured brushes. In uniaxal crystals they form an eight-rayed star, composed of two

square crosses, inclined at an angle equal to that between the planes of polarization, every ray of which separates complementary rings. In biaxal crystals they compose two pairs of rectangular hyperbolas, the angle between whose asymptotes is the same as that between the planes of polarization, and whose branches divide complementary rings. The two crosses or two sets of hyperbolas unite when the planes of polarization are parallel or perpendicular. But in the case under consideration the rings exhibited by crystals will not be traversed by any brushes. Uniaxal crystals will exhibit circular rings without a cross; and biaxal crystals will exhibit complete lemniscates, without any interruption from curved brushes." And it is further to be noticed, as the formula given above indicates, that the centres of the rings will be bright or dark according as the analyzer stands at 0° or 90°.

To pursue this matter further. Suppose that, the arrangements remaining otherwise as before, the analyzer be turned round; then in any position intermediate to 0° and 90° the rings will be contracted and extended in opposite quadrants until at 45° they are divided by two diagonals, on each side of which the colours are complementary. Beyond 45° the rings begin to coalesce until at 90° the four quadrants coincide again. During this movement the centre has changed from bright to dark. If the motion of the analyzer be reversed, the quadrants which before contracted now expand, and *vice versâ*. Again, if the crystal (say positive) be replaced by another (say negative), the effect on the quadrants of the rings will be reversed. This method of examination therefore affords a test of the character, positive or negative, of a crystal.

A similar process applies to biaxal crystals; but in this case the diagonals interrupting the rings are replaced by a pair of rectangular hyperbolas, on either side of which the rings expand or contract, and the effect is reversed either by reversing the motion of the analyzer, or by replacing a positive by a negative crystal, or *vice versâ*. The experiment may then be made in biaxal crystals by turning the analyzer slightly to the right or to the left, and observing whether the rings advance towards, or recede from, one another in the centre of the field. In particular, if polarizer and analyzer being parallel, the plate A have its axis in a N.E. direction to a person looking through the analyzer, the plate B its axis in a N.W. direction, and the crystal be so placed that the line joining the optic axes be N.S., then on turning the analyzer to the right the rings will advance to one another if the crystal be negative, and recede if it be positive. The mathematical expression for the intensity of the light at any point P is in this case

$$\tfrac{1}{2}(1 + \sin. 2j \cos. \theta + \sin. 2b \cos. 2j \sin. \theta),$$

where b is the angle between the principal section of C through P and the principal section of B, and j the angle between the principal sections of B and the analyzer. This shows that when the polarizer and analyzer are parallel or crossed at 0° or 90°, and consequently $j = 45°$

or 135° the expression is independent of b, *i. e.* the intensity is the same throughout circles about the centre, but that when the polarizer and analyzer are crossed we have an expression of the form

$$\tfrac{1}{2}(1 \pm \sin. 2b \sin. \theta),$$

the sign of the second term depending upon the direction in which the analyzer has been turned, and also upon the sign of θ, that is, upon the character (positive or negative) of the crystal.

The dispersion of the planes of polarization effected by the passage of plane polarized light through a plate of quartz cut perpendicular to the axis may be rendered visible by interposing such a plate of quartz between the polarizer and a uniaxal or biaxal crystal, when the analyzer is at 90°, *i. e.* when dark brushes are formed. In this case the brushes cease to be black and are tinged throughout with colour. The analyzer must, however, be turned back or forward, according as the quartz be right-handed or left-handed, in order that it may cross in succession the planes of polarization of the different coloured rays, and so produce the most vivid effects. The dispersion of the brushes by a plate of quartz may however be studied by employing an additional polarizer and quartz plate between the source of light and the whole system previously used. By turning this polarizer round we extinguish each ray of the spectrum in turn and tint the whole field with the complementary colour. The brushes will then appear to revolve about their centres as the tints vary continuously from one end of the spectrum to the other. If the polarizer be turned still farther round, the tints which had changed continuously from red to violet, or *vice versâ*, change suddenly from violet to red, or *vice versâ*, and the brushes jump suddenly back to their original position.

This last optical arrangement may be employed to examine the more important phænomena of the dispersion of the optic axes produced, not by a quartz plate between the usual polarizer and crystal, but by certain biaxal crystals themselves.

[W. S.]

GENERAL MONTHLY MEETING,

Monday, May 6, 1872.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

The following Vice-Presidents were nominated for the ensuing year:—

The Marquis of Salisbury.
The Earl of Rosse.
General Sir Edward Sabine.
William Spottiswoode, Esq. the Treasurer.

John Eric Erichsen, Esq.
Charles Howard, Esq.
John Saunders, Esq.
George Noble Taylor, Esq.

were *elected* Members of the Royal Institution.

The following letter was received from the Right Hon. H. Austin Bruce, the Secretary for the Home Department, in reference to the Address to Her Majesty the Queen, adopted at the last Meeting:—

"WHITEHALL, 15*th April*, 1872.

"SIR,

"I have had the honour to lay before the Queen the loyal and dutiful Address of the President, Managers, Visitors, Professors, and Members of the Royal Institution, on the occasion of the illness of His Royal Highness the Prince of Wales.

"I have to inform you that Her Majesty was pleased to receive the Address very graciously.

"I am, Sir,
"Your obedient Servant,
"H. A. BRUCE.

"Sir Henry Holland, Bart., &c., &c."

JOHN TYNDALL, Esq. LL.D. F.R.S. was re-elected Professor of Natural Philosophy.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Actuaries, Institute of—Journal, No. 87. 8vo. 1872.
Agricultural Society of England—Journal, New Series, No. 15. 8vo. 1872.
American Academy of Arts and Sciences—Memoirs, New Series, Vol. X. Part 1. 4to. 1868.
American Association for the Advancement of Science—Proceedings of the Meeting at Troy, New York, August, 1870. 8vo. 1871.

Antiquaries, Society of—Proceedings, Vol. V. No. 3. 8vo. 1872.
Asiatic Society, Royal—Journal, New Series. Vol. V. Part 2. 8vo. 1872.
Astronomical Society, Royal—Monthly Notices, Vol. XXXII. Nos. 5, 6. 1872.
British Architects, Royal Institute of—Sessional Papers, 1871-2. No. 8. 4to.
Chemical Society—Journal for March, April, 1872. 8vo.
Cornwall Polytechnic Society, Royal—Thirty-ninth Annual Report. 8vo. 1871.
Cox, Edward W. Esq. S.L. M.R.I. (the Author)—Spiritualism answered by Science. New Edition. 8vo. 1872.
Editors—Academy for March, 1872. 4to.
American Journal of Science for March, 1872. 8vo.
Artisan for March, 1872. 4to.
Athenæum for March, 1872. 4to.
Chemical News for March, 1872. 4to.
Engineer for March, 1872.
Food Journal for April, 1872. 4to.
Horological Journal for April, 1872. 8vo.
Journal of Gas-Lighting for April, 1872. 4to.
Mechanics' Magazine for April, 1872. 8vo.
Nature for April, 1872. 4to.
Pharmaceutical Journal for April, 1872. 8vo.
Photographic News for April, 1872. 4to.
Franklin Institute—Journal, No. 555. 8vo. 1872.
Geological Society—Quarterly Journal, No. 110. 8vo. 1872.
Girard, M. Jules (the Author)—La Photographie appliquée aux Études Geographiques. (O 14) 16mo. 1871.
Liebreich, R. Esq. M.R.I. (the Author)—Atlas of Ophthalmoscopy. Second Edition, 4to. 1870.
Lubbock, Sir John, Bart. M.P. F.R.S. (the Author)—Address to the Anthropological Institute, January 15, 1872. (Jour. Anthrop. Inst.)
Manchester Literary and Philosophical Society—Proceedings. Vol. XI. Nos. 11, 12, 13. 8vo. 1872.
Mechanical Engineers' Institution, Birmingham—Proceedings, January, 1872. 8vo.
Medical and Chirurgical Society, Royal—Proceedings, Part 34. 8vo. 1872.
Meteorological Committee of the Royal Society—Currents and Surface Temperature of the North Atlantic Ocean. 4to. 1872.
Paine, Martyn, M.D. (the Author)—Physiology of the Soul and Instinct, as distinguished from Materialism. 8vo. 1872.
Institutes of Medicine. Ninth Edition. 8vo. 1870.
Photographic Society—Journal, No. 232. 8vo. 1872.
Preussische Akademie der Wissenschaften—Monatsberichte, Jan. 1872. 8vo.
Royal Society of Edinburgh—Transactions, Vol. XXVI. Parts 2, 3. 4to. 1871-2. Proceedings, 1870-1. 8vo.
Royal Society of London—Proceedings, No. 133. 8vo. 1872.
Royal Society of Tasmania—Monthly Notices for 1870. 8vo. 1871.
Statistical Society—Journal, Vol. XXXV. Part 1. 8vo. 1872.
Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, April, 1872. 8vo.
United Service Institution, Royal—Journal, No. 65a. 8vo. 1871.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, Okt. bis Dez. 1871. 4to.
Vereins für Kunst und Alterthum in Ulm—Verhandlungen. Neue Reihe, Erstes Heft. 4to. 1869.
Victoria Institute—Journal of Transactions, No. 21. 8vo. 1872.
Walford, Weston S. Esq. M.A. M.R.I.—Papworth's Alphabetical Dictionary of Coats-of-Arms: Part 15. 8vo. 1871.

WEEKLY EVENING MEETING,

Friday, May 10, 1872.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Nevil Story-Maskelyne, Esq. M.A. F.R.S.

On Meteoric Stones.

The substantial unity of the celestial objects distinguished in common language by the names shooting or falling stars, fire-balls, and meteorites, and further the coincidence in many important respects of these with comets, and possibly with the zodiacal light, were suggestions made by Humboldt in the 'Cosmos,' which have received much confirmation from the subsequent advance of science.

The greater apparent velocity with which the ordinary meteors traverse the atmosphere as compared with that with which the less frequent larger bodies are seen to move, the marked periodicity that attends the recurrence of the former in several, and especially in two, notable cases of meteor-showers, offer an apparent contrast between these classes of meteors; it is not, however, in all probability, a real contrast, for the one class passes into the other by every gradation in the magnitude of the mass or masses of which the meteor consists, and consequently in the grandeur of the phenomena which accompany its advent. If of the material composing the ordinary falling star we have never yet been able to recognize any vestiges as reaching the earth, of the meteorite, on the other hand, the mineral collections of Europe contain numerous carefully collected specimens, which are the fragments that have escaped the fiery ordeal of the transit through our earth's atmosphere, and in these we recognize masses composed either of iron (siderites), or of stone (aerolites), or of a mixture of the two (siderolites). The phenomena associated with such falls of meteoric matter have been described in very similar language by those who have witnessed them in various parts of the world, and these accounts, whether coming from European observers or from Hindoo herdsmen (of which some were read by the lecturer), concur generally in the approach of the meteorite as a fiery mass, emanating from a cloud when seen by day and exploding often with successive detonations that are heard over a great extent of country, even in certain cases at points more than 60 miles distant, but finally reaching

the earth with a velocity little higher than what might be due to the motion of a falling body. Externally these meteoric masses are generally hot when they fall; sometimes, however, they are not so: the discrepancies in the accounts being explained by one authenticated case in which the mass was internally intensely cold, though at first hot externally. The fallen meteorite is invariably coated with an incrustation, sometimes shining as an enamel, generally black, but occasionally colourless where the aerolite is free from ferrous silicates; and this incrustation is seen to have been formed in the atmosphere, since it is found coating surfaces of fragments that have been severed by the explosions in the air.

Aerolites frequently fall simultaneously in large numbers, many thousands of them being in such cases spread over a surface of the country some miles in extent; and such showers of stones seem to have entered the atmosphere as a group, though their numbers must subsequently have been greatly increased by the division accompanying their detonation.

The explanation of the incrustation and of the cloud left by the meteorite, or out of which it seems to emerge, is found in the transformation into heat of the energy actuating a body that enters our atmosphere with a motion of 12 to 40 miles in a second. The velocity of the body is almost instantaneously arrested by the atmospheric resistance, and in a very few seconds the mass becomes, comparatively speaking, stationary. Its surface must, as a consequence, be immediately fused, and the melted matter would be flung off from it into the surrounding air, fresh surfaces continually affording new fused material to form the cloud of, so to say, silicious spray that lingers along and around the path of the meteorite.

When the mass is small,—and in the case of meteoric showers and ordinary falling stars it cannot exceed a few ounces, and may often be but a few grains,—the whole material is thus consumed, and must ultimately fall as an unperceived, because widely-scattered, dust. The meteorite is the residue that survives this wasting action where the magnitude of the mass is more considerable. The cause of the violent and often successive explosions is probably to be sought in the expansion of the outer portions of the mass while the interior retains the contracted volume due to the intense cold of space with which the meteorite enters the atmosphere.

From time to time these contending conditions of volume may, as in a Prince Rupert's drop, produce explosion, the heated shell in the case of the meteorite flying off in fragments from the internally cold inner core, which if sufficient velocity remain to the mass will undergo a recurrence of the same conditions of surface fusion and explosion. The loudness of the detonation is also probably enhanced by the simultaneous collapse of the air on the vacuum that would follow the rapidly moving mass.

The pitted surface characteristic of meteorites probably bears witness to a similar effect of unequal dilatation operating more

especially in the freshly-broken surfaces of the mass, small fragments splintering off in this way from the cold and brittle stone under the sudden influence of intense heat.

A remark made by Humboldt, that light and meteorites are the only sources of our knowledge regarding the universe external to our world, points to the true ground for our interest in the waifs and strays of extra-telluric matter that thus fall upon our globe.

In physical as well as in chemical characters aerolites resemble at the first aspect some terrestrial volcanic rocks.

The minerals of which they are composed are nearly entirely crystalline, as is evidenced by the colours in polarized light of such as are transparent. These minerals are usually aggregated with slight cohesion, and they present in by far the greatest number of cases a peculiar spherular or "*chondritic*" structure.

In these the spherules are composed of similar minerals to those which enclose them, and even contain metallic iron sometimes in microscopically fine grains disseminated through them.

A section of an aerolite was exhibited by the microscope in which some of the spherules had been broken before being cemented by the surrounding mass, and in another fissures were seen which had been filled with a fused material after one side of the fissure had slidden along the other: facts pointing to events in the history of the meteorite subsequent to its first formation.

The chemical composition and the mineral constitution of aerolites were illustrated by tables showing the elements met with in these bodies, and the minerals in which they were distributed. The former comprised about one-third of the known elements; among them magnesium, iron, silicon, oxygen, and sulphur were conspicuous; calcium, aluminium, nickel, carbon, and phosphorus coming next in importance, the basic elements of most importance by their amount being the same as those which are found by spectroscopic analysis to be present in the sun—and in those stars which have been the best examined.

The minerals most frequent in aerolites besides nickeliferous iron or troilite (iron monosulphide) and graphite, are bronzite (a ferriferous enstatite) and olivine, both the latter being essentially magnesium silicates. Augite and anorthite also occur (more particularly in the eukritic aerolites of Rose) and some minerals unknown in terrestrial mineralogy have also been met with; such are the different varieties of Schreibersite (phosphides of iron and nickel): calcium sulphide, asmanite (a form of silica crystallizing in the orthorhombic system and having the specific gravity of fused quartz), and a cubic mineral with the composition of labradorite. The crystalline form of bronzite was first determined from the crystals in a meteorite, and was found to confirm the conclusion Descloizeaux had arrived at as regards its system from observations on the distribution of the optic axes in the terrestrial bronzite and enstatite.

The question as to whence the meteorites come is one that we are not yet in a position to answer with certainty. The various hypotheses

which suppose for them an origin in lunar volcanoes, or in our atmosphere, or again in a destroyed telluric satellite, or that would treat them as fragments of an original planet of which the asteroids are parts, or as masses ejected from the sun; all these hypotheses seem to be more or less precluded by the known velocities, the retrograde motion so frequently characterizing meteors and meteorites, or else by the chemical conditions that for instance are involved in the passage of the meteorite through the sun's chromosphere. Whether meteorites move or do not move in circumsolar orbits is at present impossible to say; because, while with our incomplete knowledge we cannot to-day attach the character of periodicity to any known class of meteorites, we are not justified in founding any conclusion on a negative result with so limited a foundation.

But even if all or some of them may have been, on their encountering the earth, members temporarily or permanently of the solar system, we may with considerable probability consider them as having originally entered our system from the interstellar spaces beyond it. Such at least must be our conclusion if we are to admit the unity of the whole class of phenomena of meteorites and falling stars. For, since the orbits of the two best known meteoric streams, those namely of August and November, have been identified with the orbits of two comets, and since in regard to one of these (that of November) Leverrier has shown, with great probability, that as a meteoric cloud it entered and became a member of our system only some 1700 years ago in consequence of the attraction of Uranus, while the August meteoric ring only differs in this respect from it, that it had at a much more remote period found an elliptic orbit round the sun; we are constrained on the assumption with which we started to recognize also in a meteorite a visitor from the regions of remote space. And so far as it goes, the observation by Secchi that the November falling stars exhibit the magnesium lines is in harmony with this view.

It may, however, further be said, that the tendency of scientific conviction is in the direction of recognizing the collection towards and concentration in definite centres, of the matter of the Universe, as a cosmical law, rather than the opposite supposition of such centres being the sources whence matter is dispersed into space. In the meteorites that fall on our earth (certainly in considerable numbers) we have to acknowledge the evidence of a vast and perpetual movement of matter in space, about which we can only reason as part of a great feature in the Universe which we have every ground for not supposing to be confined within the limits of the solar system.

That this matter, whether intercepted or not by the planets and the sun, should to an ever-increasing amount become entangled in the web of solar and planetary attraction, and that the same operation should be collecting round other stars and in distant systems such moving "clouds" of star-dust as have been treated by Schiaparelli, Leverrier, and other astronomers, or individual masses of wandering stone or iron is a necessary deduction from the view that we have assumed regarding

the tendency of cosmical matter to collect towards centres. But in order to trace the previous stages of the history of any meteorite, and, in particular, to determine the conditions under which its present constitution as a rock took its origin, we have only for our guide the actual record written on the meteoric mass itself; and it is in this direction that the mineralogist is now working.

But the progress is necessarily a gradual one. We may indeed assert that the meteorites we know have, probably all of them, been originally formed under conditions from which the presence of water or of free oxygen to the amount requisite to oxidize entirely the elements present were excluded; for this is proved by the nature of the minerals constituting the meteorites and by the way in which the metallic iron is distributed through them.

And one suggestive and significant fact remains to be alluded to; the presence, namely, in some few meteorites of combinations of hydrogen and carbon, which if met with in a terrestrial mineral would with little hesitation be assigned to an organic origin. A few grains were exhibited to the audience of such a body, crystallized from ether, which solvent had extracted it to the amount of about 0·25 per cent. from six ounces of the Cold Bokkveldt meteorite.

Similar substances have been extracted by Wöhler, Roscoe, and other chemists from this and other meteorites. It was, however, observed, as pointing to the probability of the comparatively porous meteoric stone having in this case taken up the hydrocarbon as a substance extraneous to it (possibly when in the state of a vapour), that ether extracted it entirely from the solid lumps of the meteorite; pulverization not in any way adding to the amount obtained, or facilitating in any appreciable degree the separation of the substance.

[N. S. M.]

WEEKLY EVENING MEETING,

Friday, May 17, 1872.

The Earl of Rosse, B.A. D.C.L. V.P.R.S. Vice-President, in the Chair.

F. A. Abel, F.R.S. Treas. C.S.

On the more important Substitutes for Gunpowder.

No progress has been made since 1868 in the application of explosive agents, other than gunpowder, to artillery purposes. Somewhat limited experiments with gun-cotton in small field guns led the Austrians, about ten years ago, to the erroneous conclusion that they had conquered the difficulties attending the safe employment of gun-

cotton, applied according to Von Lenk's system, in guns of small calibre at least. Considerable progress was made in England during 1867–68 towards the production of a thoroughly safe cartridge of compressed gun-cotton for field guns, but much evidently still remained to be done when the experiments were suspended before a sufficiently uniform action of such a cartridge would be secured. The difficulties which have since been encountered in moderating and regulating the explosive force of gunpowder when employed in the large charges which have now to be used in heavy guns show how remote is the prospect of successfully applying explosive agents more rapid and violent in their action than gunpowder to artillery, with the exception perhaps of the smallest calibres.

The numerous attempts to apply substitutes for gunpowder in small arms have in some instances been attended with partial success. Many of these have differed greatly from each other, but all of them have been more rapidly explosive and therefore more violent and destructive in their action than powder. Gun-cotton in one form or another has been repeatedly made the subject of patient experiment as a material for use in small arms. The first attempts at its employment, made soon after its discovery in 1846, were disastrous in their results, and the success which long afterwards was believed to have been achieved, by Von Lenk's indefatigable labours, in the production of a safe and uniform cartridge, ingeniously constructed of layers of braided gun-cotton threads, was not confirmed by experience. Several methods of reducing the rapidity and increasing the uniformity of action of gun-cotton in small arms have since been made the subject of experiment in England. Some of these, which consisted in the uniform dilution of gun-cotton either with ordinary cotton or with less explosive varieties of the material, have furnished fairly-efficient cartridges for sporting purposes, which, though wanting much in uniformity, have established for themselves a superiority over gunpowder in regard to freedom from smoke and fouling, and one or two other qualities. But the only direction in which substantial prospect of success has hitherto attended the use of gun-cotton cartridges in arms of precision has been the conversion of gun-cotton pulp by moderate compression into very uniform masses, the rapidity and violence of explosion of which were retarded by impregnating these throughout with some perfectly inert material, thus enveloping each particle of gun-cotton in a film of non-explosive substance. The experiments upon this system of preparing cartridges have not been pursued for the last four years, but some very good targets at 500 yards were made with the service Enfield and Snider arms in 1867 and 1868, with cartridges of gun-cotton pulp impregnated with small quantities of paraffin or stearine. India-rubber has also been employed in a similar manner as a retarding and at the same time water-repelling agent. Considerable success has recently attended repeated trials with a species of gunpowder devised by Mr. Punshon, in which the principle of dilution of gun-cotton is also adopted, this

explosive being incorporated with sugar and saltpetre. A preparation of somewhat similar nature, containing as one component an imperfect kind of gun-cotton made from sawdust, and known as Schultze's powder, has also acquired some reputation, though scarcely bidding fair to compete in uniformity of action with the excellent gunpowder now manufactured for breech-loading rifles.

The application of powerful explosive agents in shells would appear at first sight to present little difficulty, beyond that involved in the selection of a material which presents a decided advantage in point of disruptive power over gunpowder, without exerting an excessive disintegrating action upon the mass of the shell and thereby effecting its comparatively harmless dispersion. An important obstacle to the employment of many of the more powerful explosive agents as charges for shells exists in their liability to premature explosion by the concussion which the shell has to sustain upon the discharge of the gun. Attempts to employ gun-cotton in shells have several times been attended by such premature explosions, more or less disastrous to the guns used. In comparing the relative sensitiveness to explosion by a blow or concussion of different compounds and mixtures, their submission, under precisely similar conditions, to the blow of a weight falling from measured heights has furnished satisfactory results, and by pursuing this line of experiment, very useful data have been obtained. The conditions which are variable in such experiments require, however, very careful regulation, as the results attained may be modified to almost any extent by variations of such elements as the area of the surface of material struck, the thickness of the mass, its mechanical condition (whether in coarse or fine powder, or in a rigid or plastic mass), the nature of the materials composing the weight and the anvil or support. Thus, a layer of mealed powder 0·05 inch thick placed between two flat brass plates one inch square is exploded by the blow of a 50-lb. weight falling from a minimum height of 36 feet, while a layer of the same thickness placed between brass plates like the preceding, but 0·5 inch square, is exploded by a fall of the 50 lb. from a height of about 9 feet. Small flat charges of fine-grain powder weighing five grains, enclosed in tinfoil and placed upon a steel support, were always exploded, in ten successive experiments, by the fall of a steel 25-lb. weight from a height of 2 feet; when a brass support was substituted for that of steel, only four charges out of ten were exploded; when both weight and support were of brass, only two out of ten were fired, and when the support and weight were of lead or wood, no explosion was obtained even when the weight fell from a height of 40 feet. Again, a nitro-glycerine preparation, of which a layer of a particular thickness, placed between brass plates resting on a solid block of iron, was exploded by a fall of a 50-lb. weight from a height of 2 feet, was not exploded by a fall of 40 feet, when the lower brass plate was fixed upon a wooden block, the upper plate being attached to the weight by means of a small block of wood.

Of the many explosive preparations more violent than gunpowder

which have been submitted to comparative experiments of the above nature, a mixture of ammonium picrate with saltpetre proved the least sensitive to explosion by blow, thus contrasting remarkably with the violently explosive mixtures of potassium picrate, which have been made the subject of experiment in France. Picric acid, which is now manufactured extensively for tinctorial purposes by the action of nitric acid upon phenol or carbolic acid, has been known since the end of last century as capable of furnishing explosive mixtures. Some of its salts, such as those of potassium and barium, are of themselves explosive, and furnish violently detonating mixtures with saltpetre and potassium chlorate. The mixtures of ammonium picrate with these salts, though less powerful in their action, are considerably more so than gunpowder, and the saltpetre mixture which has been called *picric powder*, has been shown by extensive experiments to be as safe as powder in manufacture and use, and as permanent in character; shells charged with it have been repeatedly fired from guns of large calibre, with heavy powder-charges, and there appears strong reason for placing confidence in this material as fulfilling the conditions, in regard to safety and power, of a very efficient powder for shells.

Most important progress has been made, during the last few years, in the application of explosive agents, more violent than powder, to mining and quarrying and to various civil and military engineering purposes. The hope of realizing the advantages which would be secured, especially in point of economy in time and labour, by an explosive agent combining increased power with other essential qualities of a practically useful material, has led to the production of a great variety of preparations designed to serve as powder-substitutes in its industrial applications. The powerful oxidizing agent potassium chlorate has thus been applied in numerous ways; and some of its preparations, of comparatively safe nature, such as Horsley's and Ehrhardt's powders, have afforded prospect of advantageous employment, though at present it appears doubtful whether they can enter into competition with the more violent explosive agents which have within the last six years become formidable rivals of powder.

Gun-cotton was gradually growing into extensive use, as a mining agent in England, within a year of its discovery by Schönbein; but its application was arrested for many years by the explosion which occurred at Messrs. Hall's works in 1847. Between that period and 1863, when its manufacture was resumed in England, the material was made the subject of elaborate experiment in Austria. Important improvements were eventually introduced in its production and purification by Baron von Lenk, and it was converted by him into a form decidedly better adapted for mining purposes than the gun-cotton wool, namely, that of a compact rope, having a central perforation, and cut into suitable lengths for mining charges. An extensive series of experiments, instituted in Austria with gun-cotton in this form, appeared satisfactorily to establish its superiority over gunpowder, bulk for bulk, as regards rending and shattering effects, when used in hard

rock, or when confined in strong cases. The absence of smoke, and the considerable reduction in weight of the charge required to produce a particular result, were also important advantages, substantiated by the experiments of Sir Edward Sabine's Committee on Gun-cotton, and by the results of practical trials in this country. The benefits, in point of economy and efficiency, derived from the employment of the rope-charges, were very greatly increased when the system was devised of reducing the gun-cotton fibre to pulp, and converting it by powerful compression into compact homogeneous masses, having about double the density of the rope. Important consequences of the large reduction in the space occupied by gun-cotton, when used in this form, were, the very considerable increase in the amount of tamping which could be used in blast-holes, and the greater concentration of the force applied; the destructive effects in hard rock were consequently much augmented, and the blast-holes could be placed farther apart, and reduced in dimensions. Large charges of compressed gun-cotton occupied so much less space than the rope-charges, and were so considerably lighter than powder-charges, that the material became specially valuable for submarine operations. Other peculiar advantages were presented by the compressed material; thus, its cost of production was greatly reduced, because cotton-waste could be employed in its manufacture, and because its conversion into the required forms required comparatively little time; its purification was more complete, as the finely-divided fibre was much more readily washed than the long fibre required for furnishing rope-charges; and its uniformity was much greater, because the products of a large number of successive small operations were intimately blended together in the pulping and washing processes.

One point of inferiority, exhibited by gun-cotton in the form of wool, or rope, was not at first remedied, but rather increased by its conversion into compressed masses. This consisted in the necessity for very strong confinement of the material, for the proper development of its explosive force. If used just like gunpowder, in rock which was soft or contained fissures, its destructive effects were very imperfect, and under these circumstances very irritating and unwholesome vapours were produced by its ignition. Large charges used in military operations or for the removal of great masses of rock required to be confined in very strong receptacles. Hence, although the use of compressed gun-cotton in ordinary blasting and quarrying operations in hard rock soon began steadily to increase, and though its value for submarine operations was undoubted, it continued for some time to be uncertain in its effects when employed in a particular class of mining operations and when used for military engineering purposes. After it was found, however, that compressed gun-cotton could be exploded through the agency of detonation, the metamorphosis being thereby so rapidly transmitted throughout a mass as to render any confinement unnecessary, its application became extended to new and important purposes, and it became closely allied, in regard to the

results which it furnished, to the companion explosive agent, nitro-glycerine, which has been raised within the last nine years from the obscure position of a useless chemical product to an exalted rank among practically useful explosive agents entirely through the labours of Mr. Alfred Nobel.

In 1863 Mr. Nobel first attempted to apply nitro-glycerine by impregnating the grains of gunpowder with it, and igniting the mixture in the usual way. But this mode of proceeding being uncertain in its results, he concluded that the certainty of exploding nitro-glycerine would be increased if a small portion of the mass were raised by some special contrivance to the heat at which it would violently explode, the explosion being thereby transmitted through the entire mass. He suggested several devices for producing what he termed the initiative explosion of a portion of the charge, but the most simple and successful one consisted in the employment of a large percussion cap, by the explosion of which the adjacent particles of nitro-glycerine were suddenly exposed to a high temperature and to a sharp concussion. This was the first instance in which the violent explosion or detonation of compounds of this class, known not to explode violently unless strongly confined, was accomplished through the agency of initiative detonation. Compressed gun-cotton was soon afterwards observed by Mr. Brown to behave similarly, and in the course of an investigation instituted by Mr. Abel into these and other phenomena exhibited by explosive agents, this susceptibility to violent explosion, without the aid of confinement, through the agency of a detonation, was found to be shared by all explosive compounds and mixtures, even including gunpowder, though the force and nature of the detonation required to develop the explosive metamorphosis differed very considerably with different substances. Numerous interesting results were obtained which showed that the development of detonation was at any rate not simply due to the exposure of particles of the substance to a very high temperature or to the suddenness and violence of the concussion to which they were submitted.

Mr. Nobel's discovery of a simple method of exploding nitro-glycerine placed this substance at once at the head of practically useful explosive agents in point of power; and the success with which he developed the manufacture of nitro-glycerine soon rendered this remarkable liquid available for extensive technical uses. Its value as an explosive agent for mines, especially where very hard rock had to be operated upon, was speedily established in Sweden, Germany, and some other countries; but its extensive manufacture and employment was very soon followed by numerous fearful accidents, which appear mainly ascribable to the physical peculiarities of the substance. Its liquid nature, though valuable in special instances of its employment, constituted a serious obstacle to its safe transport, storage, and use: its liability to leak from receptacles in which it was stored and the great susceptibility to explosion, by friction or blow, of portions which escaped confinement, were unquestionably fruitful sources of acci-

dent. The precaution of storing and transporting nitro-glycerine in the form of a non-explosive solution in wood-spirit, did not prove to be a trustworthy safeguard against accident, as the spirit was liable to evaporate or become weaker, and thus allow the nitro-glycerine to separate from it to a sufficient extent to re-establish danger.

A very simple expedient, which Nobel devised in 1867, soon enabled him to provide the miner with nitro-glycerine in a very convenient and comparative safe form. The observation that this substance could be exploded by detonation as readily (and even with greater certainty) if mixed with solid substances, which might be perfectly inert in character, as when used in the liquid form, led him to the production of solid but plastic preparations of this substance, to which he gave the name of *dynamite*. The earliest of these supplied to the public, and the one which, in its present improved form, is superior to any other known nitro-glycerine preparation in point of safety in storage, consists of 75 parts of nitro-glycerine held absorbed by 25 parts of a porous infusorial silicious earth, existing abundantly in Germany, and known as "Kieselguhr." This mixture was first supplied as a loose unctuous powder, from which the nitro-glycerine appeared to have a tendency to separate; it is now converted by pressure into small cylindrical rolls or charges wrapped in parchment paper, from which, as far as the lecturer's observation goes, the liquid does not appear liable to exude, even upon prolonged exposure to elevated temperatures, though immersion in water will cause a separation of nitro-glycerine after some time. Many experiments were made in Paris during the siege, by MM. Girard, Millon, and Vogt, to ascertain what absorbent materials could be best applied to the manufacture of dynamite, in the absence of Kieselguhr; precipitated silica or aluminia, sugar, and several other substances were found to be suitable media, but the material originally selected by Nobel was undoubtedly superior to all in its power of absorbing and retaining a very large amount of nitro-glycerine.

The preparation of dynamite by Nobel was speedily followed by the production of other nitro-glycerine preparations, in some of which, such as Dualine, Horsley's mining powder, and Glyoxiline, explosive substances are used as the media for absorbing nitro-glycerine. Nobel himself prepares a less violent form of dynamite by mixing nitro-glycerine in smaller proportions with powder, saltpetre, and resin or coal. A preparation which is perhaps intermediate in value between this and the "Kieselguhr" dynamite is manufactured by Messrs. Krebs, of Cologne, who have called it *Lithofracteur*. As far as its composition has been made public it is known to contain less nitro-glycerine than Nobel's ordinary dynamite; the absorbent material includes silicious earth and sand, saltpetre, coal, and sulphur. Some good practical experiments, made with this preparation at the Nantmawr and Breidden quarries, near Shrewsbury, satisfactorily demonstrated its safety in transport and use, when in its normal condition. In this, as in all other nitro-glycerine preparations con-

taining solid materials of inferior absorbent power to Kieselguhr, freedom from liability to a partial separation of the liquid can obviously be only secured at a sacrifice of explosive power, by reducing the proportion of the nitro-glycerine, and it is difficult to conceive that the consequent loss in power can be compensated for by introducing solid explosive materials less violent in their action than nitro-glycerine as components of the preparation.

Nobel's dynamite, though obviously inferior to the pure nitro-glycerine in explosive power, weight for weight, is still one of the most violent explosive agents now in practical use. In regard to power, it seems to be on an equality with compressed gun-cotton, and the results of experience in mines and quarries appear to warrant the conclusion that in the class of work where these agents can be applied to the greatest advantage, their power is about six times that of blasting powder.

The advantages in point of saving of time and labour attained by the use of these violent explosive agents in tunnelling and other operations in hard rock are very important; they are also specially valuable in submarine operations, in the breaking up of large masses of rock or of large castings and forgings, in the rapid destruction of military works, bridges, and other structures, the clearing of forests, the removal of ice obstructions, &c. Their special value is partly due to the comparatively small weight and bulk of the charges of gun-cotton or nitro-glycerine preparations required to perform the work (which in many instances could scarcely be accomplished, even by extremely large quantities of gunpowder), and partly from the facility and expedition with which these explosives can be brought into operation through the agency of detonation. Hard tamping or strong confinement is superfluous, and in many instances operations of demolition and disintegration may be effectually carried out, though with some waste of power, without any confinement whatever of the explosive agent.

Dynamite and similar nitro-glycerine preparations possess two defects in common. One arises out of the poisonous nature of the liquid, which is readily absorbed into the system, producing severe headaches and other unpleasant effects, which, however, are said to diminish in severity and even to disappear by continued use of the material. Experience has scarcely been yet acquired regarding the ultimate influence upon the life of those constantly engaged in manufacturing nitro-glycerine or using its preparations. The form in which dynamite is now furnished appears to reduce to a minimum the liability of those employing it to be injuriously affected. The other defect arises out of the readiness with which nitro-glycerine freezes at a comparatively high temperature, especially when mixed with solid substances. The occurrence of several very serious accidents during manipulations with frozen nitro-glycerine, and the fact that the sensitiveness of a substance to explosion by a blow is in direct proportion to the rigidity of its particles, favoured the con-

clusion that nitro-glycerine was specially dangerous when frozen; there is, however, no doubt that the substance is very much less sensitive to explosion by blow or concussion in that state—a fact which must be due to the greater expenditure of heat requisite for conversion of the solid than of the liquid into gas. The accidents with frozen nitro-glycerine and its preparations appear to have arisen from reckless handling in consequence of over-confidence in the safety of the material, and also from want of care or ignorance in carrying out the necessary operation of thawing the explosive agent before applying it in the usual manner. The necessity for thawing dynamite and similar preparations, which remain frozen even at 50° F., is a serious inconvenience, which further experiment in the use and properties of these materials may perhaps set aside.

Plastic preparations of nitro-glycerine possess some advantages over compressed gun-cotton in several ordinary blasting operations. The application of a little pressure after the charge has been inserted will cause it to spread out and fill up any inequalities in the hole, and thus a larger charge may in some instances be employed than if the rigid cylinders of compressed gun-cotton were used; the latter are also liable to become jammed or fixed when they are being pushed down a rugged or irregularly-shaped hole; if the miner then proceeds to force the charge home by powerful blows, which, with reckless confidence engendered by the general safety of the material, he not unfrequently will, it may become ignited and must explode violently if force is being applied at the instant of its ignition, as it is then in the position of a strongly-confined charge. Accidents in the charging of holes are obviously less likely to occur with plastic materials. Dynamite has the additional advantage of being readily applicable in blast-holes from which water cannot be excluded, as the material resists penetration by water for a considerable period. To counterbalance these advantages, gun-cotton is perfectly innocuous, and its ready explosibility by detonation is not in any way affected by cold. When carried into the field for military purposes compressed gun-cotton is very decidedly safer than nitro-glycerine preparations; because if carts or packages containing the latter are fired into from accident or design with ordinary small-arm bullets, their contents will be violently exploded as by detonation, while the gun-cotton under the same circumstances would be simply inflamed.

Although gun-cotton and nitro-glycerine mixtures possess very important advantages over gunpowder in all applications where suddenness and violence of action are desirable, there are some directions in which they do not possess superiority over powder, and others in which they cannot replace it, irrespectively of its applications to projectile purposes. In soft rock, in earth mines, and in some blasting operations, when it is desired to *displace* large masses of earth, rock, or stone, the gradual action of gunpowder gives it decided superiority. The more violent explosive agents produce great local effects; the rock, if hard, is much shattered near the charge, and is also rent

and fissured to considerable distances, but the *displacing* effect is generally inferior to that produced by the equivalent of powder, and always very much so in earth or soft rock. Decided advantages have arisen from a judiciously-combined application of gunpowder and gun-cotton or dynamite, the more violent explosive agent being used to prepare the way for gunpowder, which is afterwards applied to the removal of the material shattered by the first sudden explosion.

The degree of safety with which explosive agents may be manufactured is an important question connected with their extensive application. The fact that the manufacture of gun-cotton as now carried on involves not the slightest risk of explosion up to the final stage, when the material has to be dried, distinguishes it from most other explosive agents. In gunpowder-manufacture liability to explosion exists throughout all operations from the point when the ingredients are mixed, and with regard to nitro-glycerine it appears that up to the present time occasional severe accidents during manufacture have been inevitable. The immunity enjoyed by gun-cotton is due to its being wet, and therefore absolutely uninflammable, throughout all stages, even after it has been compressed into cakes or disks. At this point it contains 15 per cent. of water, the expulsion of which by desiccation is unattended by any liability to explosion, or even to ignition if very simple precautions are adopted. For storing large quantities with absolute safety it is very convenient to preserve the compressed gun-cotton damp, as it is delivered from the presses. It has been thus stored for very long periods without the slightest detriment, and its non-inflammability in this condition is aptly illustrated by the fact that the perforations required in some of the charges are produced by drilling the damp gun-cotton, the drill revolving at the rate of about 600 revolutions per minute. The gun-cotton employed in some extensive experiments recently made on the South Coast had been stored damp for nearly nine months, and was dried partly in the open air and partly in a hot-air chamber, when required for use. On that occasion 6 cwt. of damp gun-cotton, packed in twenty-four strong wooden boxes, were stacked in a wooden shed and surrounded by inflammable material. The building was then fired, and soon burned fiercely, which it continued to do for about half an hour, when the fire gradually subsided, and the building and its contents were entirely consumed. The gun-cotton must have slowly burned away as the surfaces of the masses became sufficiently dry, but at no period of the experiment was there even any burst of flame, due to rapid ignition, perceptible.

Another very important consideration connected with the extensive employment of an explosive compound or mixture as a substitute for gunpowder is the question of its stability. Mixtures of saltpetre or potassium chlorate, with oxidizable substances of stable character, may be generally relied upon to equal gunpowder in their unalterable nature, under all conditions of storage and use in different climates; deterioration in explosive power by the absorption of

moisture is the only prejudicial result which generally attends long-continued keeping of such mixtures. There are a few instances, however, in which absorption of moisture may in time establish slight chemical action between the components, and thus become, not only cause of more serious deterioration, but also a source of danger; as chemical activity, once started in preparations of this kind, may gradually increase, being promoted by the heat developed, until it attains a violence resulting in the spontaneous ignition or explosion of the mass. Instances are on record of the spontaneous explosion from this cause of damp mixtures, well known to be perfectly stable when dry. Substances of organic origin, of uncertain stability, require application with much greater caution to the production of explosive mixtures, as it is possible that changes may occur spontaneously in them, or may be established by natural atmospheric fluctuations of temperature, eventually giving rise to chemical action between them and the oxidizing agent with which they are mixed. Although the stability of *compounds*, which are themselves endowed with explosive properties, may appear perfectly reliable when the substance is in a chemically pure condition, it is susceptible of being seriously affected by comparatively minute causes; hence the most scrupulous care in the production and purification of such substances is imperatively necessary; and in this respect they compare disadvantageously with gunpowder, as a want of care in its production, though it may lead to accident during manufacture, or to an inferiority of the product, will not affect the stability of the material.

Both nitro-glycerine and gun-cotton, when prepared in small quantities and carefully purified, have been long known by chemists to be subject to very gradual chemical change when exposed frequently to sunlight, and also to be liable to slow or rapid decomposition if exposed to temperatures very considerably higher than occur as extremes under natural conditions in any climate. Both substances are also well known to have exhibited great stability under normal conditions of preservation, and even when continually exposed to light; but though many specimens exist, which have remained unaltered almost since the first discovery of these bodies a quarter of a century ago, the instances are numerous in which laboratory specimens have undergone spontaneous change, with more or less rapidity.

The apparently variable nature of these substances, as regards stability, is due to the retention by them, in some instances, of small quantities of comparatively unstable impurities, derived from foreign matters contained in the cellulose or glycerine; exposure to heat or to sunlight develops changes in these, resulting in the production of acid substances; hence, if they exist in gun-cotton or nitro-glycerine, they may constitute the starting-point of decomposition when these are exposed to high temperatures or to the influence of sunlight. If they exist in gun-cotton, they will be, to some extent, enclosed in the hollow fibres, and are then only removed effectually by breaking up the latter, and long-continued washing. In nitro-glycerine, they are

held obstinately dissolved by the liquid, and their removal can also only be effectually accomplished by a long-protracted washing of the very finely-divided substance. Alkaline agents are in both cases useful in accelerating purification.

For many years nitro-glycerine was universally regarded as specially liable to spontaneous change; even samples of different quantities of several pounds each, which, within the last four years, were produced at Woolwich in immediately successive operations, all apparently under the same conditions and with the special object in view of obtaining a thoroughly purified and stable material, have exhibited great differences in their keeping qualities. They have all been preserved in the dark, side by side; some are now in their originally pure condition, others have become more or less strongly acid, and two or three have undergone complete metamorphosis into oxalic acid and other products. The manufacturing and purifying processes, as perfected by Mr. Nobel, appear to furnish more reliably uniform products than those usually obtained on a small scale, and such specimens of these products as the lecturer has had an opportunity to examine have exhibited great stability. Yet, if it were possible to trace explosions to their cause more frequently than is the case, an accidental want of stability might perhaps have been found, in some instances at any rate, auxiliary in bringing about the violent nitro-glycerine explosions which have occurred. It has, however, been already established by very extensive experience during the last three years that nitro-glycerine is a far more reliable material than was formerly believed, and that if the most scrupulous attention is paid to its purification, and is combined with vigilance during storage and use of its preparations, and the adoption of certain precautions, which have already been proved important safeguards against chemical change in materials of this class, the risk of accident is so greatly reduced as to warrant the extensive manufacture and employment of nitro-glycerine preparations under restrictions similar to such as may be deemed sufficient in the case of other explosive agents.

The causes which led to the great uncertainty with regard to stability exhibited by gun-cotton, as manufactured in the earlier days of its history, have been discussed in former discourses. The very extensive experiments and observations which were set on foot nine years ago by the Government Committee, and have been continued to this day, on the keeping qualities of gun-cotton prepared by the Austrian process, have furnished most satisfactory results. Very considerable quantities of gun-cotton, in a great variety of forms, have been stored at Woolwich for several years, and their periodical examination has failed to afford any reason whatever for doubting the stability of gun-cotton under all conditions of storage which are likely to occur. The experience thus gained applies even more favourably to gun-cotton reduced to pulp according to the system lately in use, whereby the uniform purification of the gun-cotton is more effectually secured. Compressed gun-cotton has not only been stored extensively in dif-

ferent parts of Great Britain ; it has also been exported in considerable quantities to Australia, India, the West Indies, South America, and other distant countries, and has been used under circumstances specially trying to any material of uncertain stability.

The explosions which occurred at Stowmarket nine months ago had the natural effect of dispelling from the public mind the great confidence which was becoming very generally entertained in the stability of gun-cotton. Fortunately the facts which were elicited in the course of the inquiry constituted so complete a chain of evidence as to place the first cause of the explosion beyond any reasonable doubt, and to demonstrate that it was quite independent of any want of stability of the properly-manufactured material. A supply of gun-cotton delivered from the works at Stowmarket, forming part of a quantity of which there remained a store in the magazines that exploded, was found to contain a proportion of disks in a highly impure condition. The proportion of free (sulphuric) acid existing in some of these was so considerable that it could not possibly have been left in the gun-cotton after the first rough washing which it receives immediately on removal from the acid, and *before* conversion into pulp in the rag-engines, where it is beaten up for several hours with a very large volume of water. Supposing, therefore, that the gun-cotton pulp composing these disks had been submitted to the compressing process without passing through the intermediate and principal purifying operation, it could not possibly have contained even a small proportion of the sulphuric acid discovered in the impure disks, and the same would have been the case even if the *un*-pulped gun-cotton, after the preliminary washing and ringing, could have been converted into compressed disks. It was indisputably established, therefore, that the sulphuric acid discovered in the impure gun-cotton, and which could not have been generated by any decomposition of the substance, must have found its way into the finished material in some manner totally unconnected with the process of manufacture, and that no amount of carelessness in manufacture, even to the extent of partial omission of the purifying processes, could have led to the existence of the acid found in the impure gun-cotton. That this impurity was sufficient to establish rapid change was sufficiently proved by the condition of some of the disks; and that this chemical change, accelerated as it was by the great heat of the weather at the time, gave rise to a development and accumulation of heat inevitably culminating in the ignition of some portion of the stored gun-cotton, was readily demonstrated by simple experiments with some of the impure disks themselves. But although the *ignition* of the store of gun-cotton in the lightly-built magazines at Stowmarket was completely accounted for, the very violent character of the explosions, and especially that of the second explosion of a small store, which was burning for a considerable time before its contents detonated, were results quite unexpected to those well acquainted with the properties of gun-cotton in the compressed

form. Many practical experiments had demonstrated that it might be submitted to extremely rough treatment without any risk of explosion, and single packages of the closely-confined material had been repeatedly ignited, from within and without, no other result than an inflammation and a rapid burning of the gun-cotton having ever occurred. These demonstrations of the apparent immunity from explosive properties of compressed gun-cotton, unless very strongly confined or fired by detonation, appeared fully confirmed by the results of a somewhat extensive experiment made at Woolwich a year ago with gun-cotton packed in firmly-closed wooden boxes, of the kind which Government proposed to use for storing the material. Eight such packages, each containing 28 lbs. of gun-cotton, were enclosed in a pile of similar boxes loaded to the same weight, and the contents of the centre box were ignited: no explosion resulted, and the contents of some of the boxes even escaped ignition. A second experiment, in which the centre box was surrounded by inflammable matter, so that a fierce fire burned within the heap for many minutes before the gun-cotton ignited, was also unattended by any approach to an explosion. The apparently conclusive nature of these experiments undoubtedly encouraged a false confidence in the non-liability to explosion of stores of gun-cotton in the event of accidental ignition, and the Stowmarket catastrophe demonstrated the imperative necessity for a more extensive investigation of the subject. The results of some experiments recently instituted near Hastings by the Government Committee on gun-cotton have served to throw great light upon the manner in which the explosions at Stowmarket were brought about. In the first instance, twenty-four boxes (containing 6 cwt. of gun-cotton) of the kind used in the Woolwich experiment were stored upon tables in a small wooden shed of light structure, and a heap of shavings and light wood was kindled immediately beneath the boxes, two of which were left partly opened. After the fire had been burning for about seven minutes the gun-cotton inflamed and continued to burn with very rapidly-increasing violence for nine seconds, when a sharp explosion occurred; a very similar result was furnished by a second experiment, in which the same number of boxes of gun-cotton was stored in a small magazine of stout brickwork. By subsequent comparative experiments it was judged that a considerable proportion of the gun-cotton had been burned in both instances before the explosion occurred, but these were nevertheless of such violence as to produce large craters in the shingle on the site of the buildings and to project the *débris* with much force to considerable distances. Two repetitions were afterwards made of the first experiment, in wooden sheds of similar structure, and with corresponding quantities of gun-cotton similarly arranged in boxes of the same size, and fastened down just as securely as those in the former experiment, but the boxes were made of somewhat thinner wood and were constructed less strongly. In neither of these experiments did an explosion occur. In the one instance the fire was burning in the building for more than half an

hour before the gun-cotton became ignited, and three minutes after the first great blaze had subsided there was a second blaze of gun-cotton. Although the latter must have been exposed to intense heat, no explosion was produced. In the second experiment the gun-cotton burned in three successive portions, the last having been exposed for many minutes to very fierce heat, yet burning non-explosively. The first two of these experiments demonstrated that if, in a store containing packages of gun-cotton in somewhat considerable number, the material became accidentally inflamed, the intense heat developed by the burning gun-cotton in the first instance might raise some portion, still confined in boxes, to the inflaming point, and that then, the mass of the confined gun-cotton being in a heated condition, the ignition would proceed with such rapidity as to develop the pressure essential to explosion while the gun-cotton was still confined, the resulting explosion being instantaneously transmitted to other boxes. When the magazines at Stowmarket exploded, a large volume of flame was observed to precede the explosion by a very distinct interval. The two other experiments described appear to demonstrate that *with such quantities* of gun-cotton as were stored in the experimental sheds, the fact of the material being confined in boxes of comparatively light structure constitutes a safeguard against explosion, the reason being that the weaker packages are opened up by comparatively feeble pressure from within, hence when the contents of a box become raised to the inflaming temperature, or become ignited by the penetration of flame to the interior, the pressure developed by the first ignition is not sustained by the box to a sufficient extent or for a sufficient time to bring about explosion.

On the occasion of the Stowmarket accident there were two store-sheds containing gun-cotton packed in boxes of light construction, which were ignited by the first explosion and burned out without exploding, while a third, which contained gun-cotton packed in the strong Government boxes, exploded with great violence after having been in flames for some time.

Simple experiments demonstrate that if any explosive compound or mixture be ignited when in a heated condition, it will burn with a violence proportionate to the temperature to which it has been previously heated; if this be near the exploding point, explosion must ensue, which will be violent in proportion to the strength of confinement of the material. A practical demonstration of this was furnished by an explosion which occurred at Woolwich in 1866. Several very strong packages (metal lined-cases) filled with Von Lenk's gun-cotton, some of which had been purposely left impure, had been exposed for seven months to artificial heat in a strong brickwork chamber, heated by steam. The impure gun-cotton in some of the packages was then known to be in a decomposing state, but the experiment was continued, and eventually spontaneous ignition occurred and at a time when the boxes were heated to the maximum temperature. The result was a violent explosion of all the packages; the very strong confinement and

the heated condition of the gun-cotton which ignited, added to its being at the time in a state of chemical activity, determined its explosion, and the explosion of the other packages was a necessary consequence of the violent concussion to which they were exposed.

There can be no doubt that the results of the recent experiments and of those made last year, as also the results of the Stowmarket accident, have to be considered in relation to the *quantities* of gun-cotton operated upon, as well as to its confinement. The confinement of the *eight* strong packages by the layers of boxes which surrounded them on all sides in the Woolwich experiment, was probably quite as great as that afforded by the light and roomy shed in which the twenty-four boxes of the same kind were placed in double layers, in the South Coast experiments; yet in the latter case an explosion was developed, and not in the former with the smaller quantity. In the South Coast experiments, with 6 cwt. of gun-cotton, the explosions occurred eight seconds and ten seconds after the ignition of the gun-cotton; in the Stowmarket magazine, where several tons of gun-cotton were stored, the explosion appears to have *almost* immediately followed ignition; it must be borne in mind, however, that in this case much of the gun-cotton was very closely confined by the large number of surrounding packages, and that the temperature of the gun-cotton was already raised considerably throughout by long-continued very hot weather. Both of these circumstances must have greatly favoured the very rapid development of explosion, independently of the much more intense heat generated by the rapid spreading of fire through a large proportion of the gun-cotton.

The satisfactory results obtained in the South Coast experiments with the lightly-constructed boxes, with employment of 6 cwt. of material, appear to have received confirmation from the result of an accident which occurred in 1869 at Penryn, when a magazine of brickwork containing 20 cwt. of compressed gun-cotton, packed in boxes of light structure, was burned down without any explosion. But it is nevertheless very possible that a similar result would not be furnished by several tons of gun-cotton similarly packed; the much higher temperature which would be developed in that case by the first spreading of the fire, and the additional confinement, due to the larger number of packages, might combine to develop conditions favourable to the violent explosion of some portion of the mass, though no doubt a much larger proportion would burn non-explosively than if strong boxes were used. While, therefore, in storing *dry* gun-cotton, the probability of violent explosions resulting from the accidental ignition of a magazine may be considerably diminished, or at any rate the violence of a possible explosion much reduced, by storing the material in packages of which some portions will yield readily to pressure from within, or by adopting any other storage-arrangement whereby the rapid penetration of flame or heat between the compressed masses is promoted, it must be considered as conclusively established by the last twelve months' experience that such

regulations as experience and prudence have rendered essential in connection with the storage of gunpowder and other explosive agents, must also apply to the storage of compressed gun-cotton when in the dry state.

The rapid development which has taken place within the last few years in the industrial applications of powerful explosive agents bids fair to continue. In illustration of this a brief reference may be made to some recent interesting results arrived at by Dr. Sprengel, who has observed that mixtures of liquid oxidizing agents (such as nitric acid) with liquid or solid oxidizable substances, may be made to detonate, as also mixtures of readily oxidizable liquids with solid oxidizing agents. Thus, mixtures of picric acid or of nitrobenzol with nitric acid, or of chlorate of potash with bisulphide of carbon, may be readily detonated, and are more or less violently destructive in their action.

Important advantages, in point of power in the one instance and of economy in the other, appear to be promised by the production, in compressed masses, of mixtures of gun-cotton pulp with *considerable* proportions of chlorate of potash or saltpetre. Even the efficiency of gunpowder itself as a mining agent has been decidedly augmented in some directions by the lecturer's observation that it is susceptible of violent explosion by detonation, like all other explosive preparations, and that strong confinement is consequently not essential to the development of its full explosive force. This observation has proved to be especially valuable in connection with submarine operations, for which the charges of gunpowder need no longer, as formerly, be confined in cases of great strength. There are several other directions in which the study of the behaviour of explosive agents, under conditions compatible with their practical application, promises to be fruitful of important results.

[F. A. A.]

WEEKLY EVENING MEETING,

Friday, May 24, 1872.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

PROFESSOR W. K. CLIFFORD, M.A.

On Babbage's Calculating Machines.

[No Abstract received.]

WEEKLY EVENING MEETING,

Friday, May 31, 1872.

Sir Frederick Pollock, Bart. M.A. Manager, in the Chair.

Professor Edward J. Poynter, A.R.A.

On Old Art and New Art.

We are all familiar with the argument which, while it admits that the great artists of history, whether Greek or Italian, may have conceived and realized an ideal of the human form which in these days we cannot pretend to rival, yet insists that ideas on this subject are, to use a modern vulgarism, "played out"—that we want our art to be more in accordance with the spirit of the age, which is an age of realities and progress, that our art must be above all realistic, and show us nature as we see her around us, and that it must also progress and keep pace with the advancement of science and education so as to give us something new. I do not say that the argument put in this broad and coarse form exactly expresses the opinion of any really thoughtful persons, but it is at the root of much that is written and said on the subject, and that by persons who have the command of a great deal of art-education in this country, and who at all events imagine that they have well considered the subject; and it is plausible enough to require refutation, for it is apparently not obvious to everyone at first sight where the great distinction lies between science and art which should restrain the latter within certain impassable boundaries, while there is apparently no limit possible to the discoveries and novelties of which the former is capable; and persons who hold these views do not see how essential it is to the very existence of an art that it should have an ideal of beauty—that, as it can only appeal to our minds or hearts through our senses, unless it does so on some principle of choice or selection, we gain no more from it than we gain from the observation of nature itself—that the human form and face, containing as they do the highest qualities of beauty which nature presents for our admiration, the study of them with a view to rendering the most complete abstract expression of their beauty and vitality, is the highest to which an artist can devote himself—that the aspects not only of human but of all natural beauties, being the same in all ages, there is no new discovery to be made in the matter; and so, not seeing, or caring to see, this, they cannot judge how near perfection in the attainment of this ideal the art of the past arrived, or how nearly it reached the limits outside of which it is incapable of further development,

and, not feeling competent to discuss this point, willingly concede to Greek art the achievement of a perfection for which they know the crown has always been awarded to it. What they desire, in fact, is an art which shall appeal more directly to minds incapable of appreciating its more elevated characteristics; and this, they persuade themselves, would be a higher development, because appealing to a wider range—of what?—not of sympathies, as they imagine, but in truth, of minds incapable of wide sympathies. Moreover, the argument that the progress of knowledge has given us new and more varied themes for expression, and therefore tends to produce a further development of art, must fall to the ground, unless it can be shown that these themes are of a kind that lend themselves specially to artistic treatment.

The truth is, that any attempt to rival or surpass the *chefs-d'œuvre* of the past must be made on the same conditions and in the same spirit that animated the producers of those great works. Were science to discover for us the cause of every natural phenomenon that exists, nay, were it to reach the final cause of life itself, the glow of the evening sky would be none the more or less beautiful, nor the grace of a child's movements one whit diminished or increased. These, indeed, are eternal beauties and unchangeable, and they are what the artist has to treat of; and though he may never be able to arrive at the complete expression of them, he can see to the end of them, for they live for ever for his continued contemplation.

I have no hesitation in saying that art has lost more than it has gained by our modern modes of thought and feeling, and that if it be asked why we cannot put away the traditions of the past, and work in the modern spirit, the answer is that the modern spirit is becoming daily more opposed to the artistic spirit, and is precisely what hampers its expression; that what is good in the art of to-day, is good in the same way, and for the same reasons, as the old is good; that we have no lights on the subject which were not also clear to the old masters, and that where we seem to have struck out a new path, we have only chosen one which they purposely and rightly rejected; where we seem to have discovered a new truth, it proves to be one beside the question.

Now there are, I think, two causes to be found for the immense difference in the aim and results of our modern work as compared with that of the ancients. I should have said rather that there are two ways in which the modern spirit is opposed to the artistic spirit; and one of these is in a noble direction, and is due to the spread of a philosophy—I might almost call it a religion—which insists on the recognition of certain qualities, moral and Divine, inherent in ideas or impressions of beauty, which recognition is necessary on the part of the artist to the production of a high form of art. The second, or ignoble way, may be broadly stated as due to the fact, that artists, from motives of indolence or interest, have allowed themselves to be led by the opinion of the public, instead of being, as of old, indifferent

to it, and themselves leading the way to a better appreciation on the part of the public, of the capabilities of art.

Now both these causes have curiously enough led to the same result; I mean they have both been instrumental in leading to a prevalent belief that the imitation of nature, or perhaps I should say the record of his impressions of nature, is the aim and purpose of the artist. It will be necessary, then, before going further, that we should inquire in what way and how far a mere imitation of nature may result in a work of art. And, in speaking of imitation, I must be understood to use the word in the sense of copying. Fuseli defines the difference between copying and imitation in this way: "Precision of eye and obedience of hand are the requisites of the former, without the least pretence to choice, what to select, what to reject; whilst choice, directed by judgment or taste, constitutes the essence of imitation, and alone can raise the most dexterous copyist to the noble rank of an artist." But it seems to me that it is impossible for an artist not to choose what he is going to paint; he may choose stupidly, but a choice of some kind he must make, so with this difference I take his definition of copying as what I mean by imitation.

When I say that the belief of which I am speaking is a very prevalent one, I judge not so much by what is said and written on the matter as by the large quantity of merely imitative work which is to be met with in our numerous art-exhibitions, and the amount of success such work meets with. Now it is quite true that an imitation of nature may be a work of art; when at its best it calls forth all the highest technical qualities of the painter, the qualities that distinguish him as a painter from the poet who describes nature in another way.

This precision of eye and obedience of hand requisite for the rendering of colour and form, include the whole art of painting, and are found in perfection only in the work of the most highly-gifted artists; but they are distinctly only the painter's qualities and exclude the mental. Moreover, being the qualities which are necessary to his existence as a painter, and without which he is nothing, they take the lowest place among the artistic faculties. But the fact remains, that a mere imitation of nature—what is called realistic painting, though I should be inclined to call it materialistic—(I have already endeavoured to explain in a lecture on "Beauty and Idealism" how a true realism is one of the highest forms of art)—the fact remains that this imitative painting may be so admirably done as to become of a high order of merit. It is the essence of portrait-painting, though for a good portrait other qualities are doubtless required. It is the essence also of landscape-painting, though for good landscapes other qualities are required; and it is all that is necessary for still-life painting. But it is only the groundwork of ideal art, and is akin to the language by which the poet expresses his thoughts. It is, in fact, the language of art, and may be used by the artist for the three purposes following: it may be used to describe the ordinary aspects of nature, and through the knowledge of these aspects

to express his selection of what he considers best worth describing; and thirdly, to give form to his imagination, which is the result of the selection and combination of the impressions he has received, the strength of our imagination depending, according to Mr. Darwin, on the strength and variety of our impressions, on the rapidity with which they are conveyed to our mind, and our power of retaining and combining them. Mr. Ruskin also explains the imaginative faculty in art to be a certain mode of regarding or combining the ideas which the mind has received from external nature; so that is the highest art which gives form to the imagination of the artist, not that which records impressions received immediately from Nature herself; and this imaginative faculty in the hands of men of great genius is of a truly creative kind, for is not the result of their combinations akin to the creation of a new world for the enjoyment, intellectual and moral, of those who can appreciate their inspirations? Has not Michael Angelo, indeed, created for us a new and superior order of beings, the grandeur of whom may indeed be felt but not defined, in whose honour writers have exhausted language in the attempt to give utterance to the emotion called up by his power; concerning whom Reynolds said, "I feel a self-congratulation in knowing myself capable of such sensations as he intended to excite;" of whom Fuseli wrote, "His women are moulds of generation, his infants teem with the man, his men are a race of giants;" and Ruskin, "His inexplicable power proceeds from an imaginative perception which is almost superhuman, which goes whither we cannot follow, and is where we cannot come; throwing naked the final, deepest root of man, whereby he grows out of the invisible and holds on his God home;" and again, he speaks of "the troops of terror torn up from the earth by the four-quartered winds of the Judgment, making the hair stand up, and the words be few"—language which indeed bears witness to the high nature of this creative power, for of none other could the like be used. So, then, the aim of all high art is—of all art (except that which professes to be portraiture) should be—to create a world in which our imaginations should be excited to the contemplation of noble and beautiful ideas; and in proportion as it fails to do this, so does it differ from the great work of the old masters, who never had any aim but this. And here is the reason why I put landscape-painting in the second rank of art; for though the impressions recorded be of the highest beauty, still it is but a record and an imitation—an imitation, it is true, coming rather under the head of Fuseli's second definition, as "being directed by judgment and taste"—an imitation most difficult of accomplishment, and requiring for its expression artistic skill of the highest order, on account of the subtle and fleeting effects which it is the delight and glory of the landscape-painter to recall to our minds. Of the same nature of this highest form of landscape is the more elevated form of portrait-painting, which aims at recording not only the features and costume, but all the noblest characteristics of the subject, but takes a second place as being a recording and not a creative art. Lower than

this must be placed what is called still-life painting, and that kind of landscape which is of the matter-of-fact portrait kind; these again, however beautiful the subject chosen for imitation may be, only appeal to us in so far as the subject in nature itself appeals to us, or pleases us according to the amount of technical skill displayed. Still lower again is that kind of realistic portrait-painting which we find in the modern French school, which presents a specious appearance of originality by ignoring the necessity for presenting the subject to us under an agreeable form, and so repels us in spite of an undoubted technical skilfulness, to which kind of realism I am not sure that I do not prefer our own fashionable school of portrait, as showing the faint tradition of a desire to ennoble the subject by the treatment: while the basest degradation of the art is revealed in that brutal rendering of noble subjects in which the French realistic painters take especial delight, where with insolent bravado everything is done with a direct intention of disgusting us, as showing the superior pretensions of an ignoble and ferocious nature over the purity and dignity with which such themes have been hitherto invested. Of this kind is the Salomé of Regnault, which it is worth while to compare with the graceful treatment of the same figure by the early Italian painters. Nearly on the same level, but not always so low, are the Dutch painters, who treat us to gross representations of drunken scenes; not so low, because the pleasure in these things is merely stupid, and not cynical, as with the French; and sometimes, as in the case of Ostade (who does not always condescend to these subjects, but as often paints for us the homely pleasures of a contented, if somewhat degraded, peasant life), there is a perception of the poetical beauty of glowing light and softened and mysterious shade, and a delicate skill in rendering them, which makes us forget the poverty and grossness of the idea. Not so, however, in the same scenes as painted by Jan Steen, whose remarkable power of rendering expression only adds to his disgrace; so that one may say that the better the art displayed in the imitation the more base is the result.

Now we shall find if we examine this *résumé* of the various styles of painting that they are all, with the exception of the higher kind of portraiture, of a comparatively modern invention. They have all come into practice since the end of the sixteenth century, up to which period art rested on that high ground of which I have spoken, being of a purely creative and ideal nature. They are, if you please, developments of that high school, but they develop from a creative to an imitative art; and as the creative necessarily includes the imitative (man being unable to conceive of anything outside his experience of nature), they must be considered to have taken a lower ground. Nor can it be said to be a new ground, for it is not to be supposed that the Florentine and Venetian painters, who painted the landscape of their backgrounds with such exquisite perception of the very essence of its beauty, never painted landscape pure and simple for want of appreciation; it was because they felt it a degradation of their art that it should rely

simply on its power of recalling impressions. Titian, indeed, has left us pictures of almost pure landscape; but they rather take the form of studies, and are at most but in the nature of a diversion from his other and more serious work, being painted but occasionally out of the fulness of his delight in the beauty of his native mountain scenery. And so of fruit and flower painting. There was no painter up to the end of the sixteenth century who would not have scorned to sit down to make a picture of a bunch of roses or a plate of peaches. In their beautiful ideal presentations of the infant Christ and His Virgin Mother they could conceive no more delightful manner of enriching their pictures with forms of beauty than by decorating them with garlands of fruit and flowers, and be sure they took as keen a pleasure in painting these most innocent beauties of nature as any still-life painter of the Dutch, French, or English schools. Herein, then, lies the difference between the old and new in art. A work of pure imitation *per se* was a thing unknown up to the end of the sixteenth century; not because it did not occur to the sculptors and painters of the old world to produce such work, but because they rejected it as not worthy of consideration, knowing well that art means a different thing. Modern art—I mean that part of it which is modern in spirit—is content with recalling the impressions which all of us who have a few shreds of poetic sensibility receive from the more obvious beauties of nature, and in rendering what is purely beautiful finds its expression in that school of landscape-painting which has reached perhaps its highest point in some of Turner's best work—its lowest in the mass of still-life, flower, and fruit painting, of which I suppose William Hunt is the most refined and skilful exponent.

With that phase of modern art which deals not with the beauty of nature, but its grossness or inanity, I for the present have nothing to do. . . . . I have said there are two causes for the great difference to be found between ancient and modern art, a noble and an ignoble one. I will begin with the first; and it seems rather startling to say that an idea of a noble kind may also be of a kind to mislead; but it is rather in the application of the idea to art, not in other considerations to which it gives rise, that the idea appears to me a fallacious one, and when I speak of a noble philosophy of art amounting almost to a religion, you cannot remain long under a misapprehension as to whom I mean by the exponent of this philosophy, or the prophet of this religion; the name of Mr. Ruskin cannot fail at once to occur to your minds. It is with no small amount of diffidence, I assure you, and with some misgiving, that I venture to differ from so gifted a writer and thinker, and especially on a point which he would, and indeed does, insist on as a vital one in all his teaching on art—with diffidence, for it is quite possible that I have missed his meaning, or do not see deeply enough into it, or that I have not grasped the whole of it—with misgiving, for he has the world enough against him already, and it may be, if indeed he is ever aware of the fact, a natural source of vexation to him to find that I, occupying a post analogous to his own, in which I am respon-

sible for the nature of all the art teaching which may fall to my share, so that my opinion in that sense must be considered of importance for right or wrong, should hold on an important point of that teaching an opinion different to his own. But I feel that this idea of his which I am about to challenge leads to so much that is false in the art of this country, to the production of so much of this merely recording work as being the aim of the painter, that I boldly run whatever risk I may in endeavouring to point out its fallacy. I may give it in his own words, as set forth in the second volume of 'Modern Painters,' where he enunciates the whole of his theory of art; and these are the words: "I wholly deny that the impressions of beauty are in any way sensual; they are neither sensual nor intellectual, but moral." And again, in a later work, 'The Crown of Wild Olive': "No statement of mine has been more earnestly and oftener contradicted than that good taste is essentially a moral quality." So I can hardly be mistaken as to the assertion, though, as I say, I may have failed to grasp the whole of his meaning. And in the sentence before the first one I have quoted he explains that "æsthesis," or the æsthetic faculty, properly signifies mere sensual perception of the outward qualities and necessary effects of bodies, which, although true, slightly degrades the meaning of the word æsthetic, which may surely be applied to our mental as well as our sensual perceptions. It is also to be remarked that the word sensual has come to be used in a lower meaning than æsthetic, as when we speak of sensual pleasures we mean rather the pleasures by which we gratify our lower appetites, so I prefer, for fear of misapprehension, the word "æsthetic." Now, in insisting on the æsthetic nature of our ideas and impressions of beauty, I labour under the disadvantage of appearing to abstract from them that elevated character which Mr. Ruskin has ascribed to them in his chapter on typical beauty. But observe, I do not in any way deny the truth—the philosophical truth—of this view of ideas of beauty; I only say it is beside the question, and may easily lead to false conclusions with regard to art. It is not that I hold the error to lie in the assertion of the moral qualities of beauty, but in the inference that the perception of those qualities is essential to the production of a true work of art, and that all good art has been produced under the influence of this perception. I can, indeed, conceive of no writing more calculated to elevate our moral and intellectual nature than the chapters in which Mr. Ruskin treats of the different forms of beauty as types of the Divine attributes, and not only these chapters, but the whole of his works from beginning to end set before us more exalted conceptions of the beauty and sublimity of nature than has ever been presented to us in words, amounting, as I have said, almost to the setting forth of a new religion of the purest and noblest type. But it is to me impossible not to feel throughout that the theoretic qualities are not inherent in forms of beauty, but exist in the mind of the spectator, and that the appreciation of those qualities may therefore be equally excited by any art which professes to represent them. For, be it observed, if the æsthetic

nature of these impressions be excluded, the seeing power of the artist, which is distinctly his æsthetic faculty, as distinguishing him from those who cannot receive impressions of beauty, is excluded also; in which case the earnest intention of the painter, his intense love of nature, his profound veneration and affection for the moral beauties to be found in the most trivial of God's works, may easily lead him to the belief that as long as he devotes himself to the imitation of these works he is setting forth the glory of the Creator in a better way than if he used his powers for the purpose of selection and combination. "Let the young artist beware of the spirit of choice: it is an insolent spirit at best," says Mr. Ruskin, as a sequel to saying that "if we can only admire this thing or that, we may be sure that our cause for liking it is of a finite and false nature." He must pardon me if I think that not only here, but throughout his works, he has not sufficiently separated two ideas; the faculty by which we may all derive a moral benefit from a humble contemplation of the infinite beauties and wonders of creation, from that artistic faculty which frames for us and forces upon us the perception and appreciation of those generic beauties which are not obvious to the unexercised mind. Ideas of beauty are doubtless capable of giving us a more elevated understanding of the nature of this world and this life the more they illustrate to us the nature of God, and so may have an ennobling effect on our whole moral nature; but this is beside the question when we are considering them in relation to art, when we have to decide not what excites in us the highest religious or moral emotions, but how far the art is capable of conveying these emotions to our mind; so that we must admit the æsthetic or sensual nature of ideas of beauty, or we should have to admit that the man in whom they excite the most rapturous emotions is of necessity the greatest artist, rather than he who is distinguished by his perceptive gift. An artist, indeed, may possess his enthusiasm for beauty in this theoretic form, but how is he to explain in his work that this is the case? It is doubtless easy to answer that the artist who enjoys this high moral perception of beauty must show it in his work if his art is good; but this leaves it open to the spectator to assert that that art is not good in which he can perceive no traces of this emotion, and thus we come back to the original proposition that the moral qualities of beauty exist only in the mind of the beholder, who can supply them to any work of art which he considers to set forth these beauties most clearly. Now I think if I have put my argument satisfactorily (and I am not at all sure that I have done so) you will begin to perceive how these ideas may lead to that kind of art which I have explained as being rather of a recording and imitative than of a creative nature.

The beauty of nature is very much more obvious in certain aspects and in some objects than in others; for one man who has the æsthetic faculty of being pleasurably affected by the beautiful forms and proportions of the Milo Venus, or Michael Angelo's Slave, or rather by the subtle distinctions of line which in nature go to make the dif-

ference between a form of high beauty and one that is of a mean or vulgar kind (the true æsthetic faculty this), there are a hundred who can feel the glory of a sunset, or the exquisite beauty of an anemone; and as the moral qualities of a typical beauty are set forth to the mind which can see them as gloriously in these last as in the first, it is not difficult for an artist to persuade himself that he is doing the most earnest and right-minded work in endeavouring humbly and patiently to imitate those beauties to the utmost of his power, and so convey to others the emotions that he feels: nay, he may go farther; he may persuade himself that there is nothing else worth doing, and that this work alone is of pure and spiritual nature, all other being sensuality, and therefore demoralizing, so that, like the ascetics of old, there is no kind of work too humble for him to devote himself to: or what is worse than this (for work done really in earnest and with a spirit of patience has always in it something of value, and I may add parenthetically, that no good work can ever be done without this spirit), the spectator may well imagine that the mind of the painter who laboriously produces for us again and again bunches of primroses and violets, is actuated by this earnest feeling and in possession of this high theoretic faculty, while indeed he has only been exerting his technical skill, and what amount of æsthetic faculty he may possess; nay, more, who has been but exercising just so much of his faculties as will enable him to make money in the quickest and readiest way. Thus it is quite possible for such a theorist, if himself destitute of the æsthetic perception, to exalt bad and fantastic work over good if he imagines it to give a higher expression to the typical nature of beauty; so that the critic who is not offended by the crude and discordant yellow and blue of one of Turner's later sunset pictures, may yet believe that he finds in it a deeper perception of the infinity and purity of the evening sky, than in his earlier and more soberly harmonious representations, and so ascribe to it an artistic value which it does not possess. Or it is not impossible for him to exalt in this way the birds' nests of William Hunt over the mighty conceptions of Michael Angelo and Phidias, if he imagines he perceives this earnest purism in the one and not in the others.

And in proof of this possibility we have only to look at the amount of artistic talent that is spent on painting little groups of flowers and fruit, or small landscape studies that might be developed into something better if the artist did not stop short with the idea, brought about by this train of thought, that he is doing the rightest work. It is not, indeed, Mr. Ruskin's fault that a noble theory has led to such poor results, for it is the misfortune of all great ideas that they degenerate most easily into cant; in illustration of which point I may relate an instance which has come under my own notice of a person who, on visiting an artist's studio, and having been shown pictures and designs of no mean order of beauty, begged to be allowed to see some of his "earnest work, his studies of leaves and flowers."

If, then, I have made myself clear, I shall be understood to have

said that the idea must be expressed in a work of art, and not merely exist in the mind of the artist, or be supplied by that of the beholder; that the moral nature of beauty is of a kind that cannot be expressed in painting or sculpture, and that therefore as far as art is concerned, ideas of beauty are, and must be, purely æsthetic, and that the theory of the moral nature of beauty is not only calculated to lead criticism astray when the æsthetic faculty is wanting, but may even induce an indifference on the part of the artist to higher forms of beauty than those which are at once obvious to the uneducated mind. Now, I will not stop to discuss another point in Mr. Ruskin's writings bearing on this subject, and to some extent akin to it, but in reality of a different character. I mean the idea which runs throughout his works of the necessity for a right state of moral feeling, not only in the producers, but in a nation or epoch, as essential to the appreciation and production of a noble and beautiful art. But I may remark that even if it can be shown that the best art has always arisen out of a pure state of national faith and domestic virtue, it does not follow that those conditions will always produce good art; if the æsthetic qualities be absent they certainly never will, and I can imagine no condition of national virtue which could cram ideas of beauty into the head of an average Englishman or Scotchman; but as far as regards the artist himself, it appears to me unquestionable that the æsthetic faculties being in two instances equal, there will be no comparison between the art of a man of a pure and noble mind and one of a mean and sordid nature. Indeed, the very highest artistic gifts seem to imply also a noble and healthy moral condition. Michael Angelo was himself one of the most simple and high-minded of men, incapable of any act of meanness, and we should find, I think, on examination that all the truly great artists and poets have been of the same nature. . . .

I have described the second cause as arising from the fact that artists from motives of indolence or interest have allowed themselves to be led by the public, instead of, as old, taking the lead in forming the public taste. . . . And here, I may quote a saying of Sir J. Reynolds as being to the point: "It is certain that the lowest style will be the most popular as it comes within the compass of ignorance itself; and the vulgar will always be pleased with what is natural in the confined and misunderstood sense of the word." And again, "In the inferior parts of the art the learned and the ignorant are nearly on a level;" so with this authority, I feel in a position to repeat my original assertion that modern art has struck out for itself a path which the ancients disdained to follow, and has developed itself in the direction which no genuine artist or man of taste would ever wish to see it developed, having lowered itself to the appreciation of a class of patrons and admirers unknown to the artists of the old world. . . .

If, then, we are to look forward to the more general production of a class of art of that high creative kind which I have dwelt on,

and which is at present decidedly in the minority in this land (though I must confess that I think there is a decided move in the right direction, and a desire for something of a more serious kind), we must, I think, in the first place, try not to encumber our minds with ideas, but rely on the cultivation of our perceptive faculties to show us the high beauties that lie beneath the surface of nature; and we shall then get rid of the notion that little pictures of fruit and flowers and landscape are anything more than studies to help us in the execution of better work, or to be done from time to time as mementos of some especially delightful aspect of nature in form or colour; and we must above all study the works of the great masters of antiquity. "Study the great works of the great masters," says Reynolds, "for ever. Study nature attentively, but always with those masters in your company; consider them as models which you are to imitate, and at the same time as rivals with whom you are to contend." . . . .

[E. J. P.]

GENERAL MONTHLY MEETING,

Monday, June 3, 1872.

The Earl of Rosse, B.A. D.C.L. V.P.R.S. Vice-President, in the Chair.

Charles Edward Beevor, Esq.
Frederick John Blake, Esq.
Maurice Barnard Byles, Esq.
C. F. Hancock, Esq. B.A.
George Augustus Huddart, Esq.
Joshua G. Kershaw, Esq.
Henry Samuel King, Esq.
Athol Maudslay, Esq.
Mrs. Ciccopiere St. Clair.

were *elected* Members of the Royal Institution.

The Managers reported that they had appointed William Rutherford, M.D. F.R.S.E. Fullerian Professor of Physiology.

The special thanks of the Members were returned to Sir Henry Holland, Bart., the President, for his fourteenth Annual Donation of Forty Pounds.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The French Government—Collections de Documents inédits sur l'Histoire de France. 1re Série: Histoire Politique: Lettres du Cardinal Mazarin. Ed. M. Chéruel. Tome I. 4to. 1872.

Accademia dei Lincei Reale—Atti. Tomo XXIV. 4to. 1871.

Asiatic Society of Bengal—Journal, No. 172. 8vo. 1871.
Proceedings, 1871. Nos. 12, 13. 1872. Part 1. 8vo.

Astronomical Society, Royal—Monthly Notices, Vol. XXXII. No. 7. 1872.

Bell, I. Lothian, Esq. (the Author)—Chemical Phenomena of Iron Smelting. 8vo. 1872.

Bombay Branch of the Royal Asiatic Society—Journal, No. 27. 8vo. 1872.

British Architects, Royal Institute of—Sessional Papers, 1871-2. Nos. 9, 10. 4to.

Browning, Ernst, Esq. M.R.I. (the Author)—Exposition of the Laws of Marriage and Divorce. 8vo. 1872.

Browning, George, Esq.—Footprints. Poems translated and original. 16mo. 1871.

Chemical Society—Journal for May, 1872. 8vo.

Comitato Geologico d'Italia—Bollettini. 1872. Nos. 1, 2. 8vo. Firenze.

Edinburgh Royal Observatory—Astronomical Observations, Vol. XIII. (1860-9). 4to. 1871.

Editors—Academy for May, 1872. 4to.
American Journal of Science for April, 1872. 8vo.
Athenæum for May, 1872. 4to.
Chemical News for May, 1872. 4to.
Engineer for May, 1872. 4to.
Food Journal for May, 1872. 4to.
Horological Journal for May, 1872. 8vo.
Journal of Gas Lighting for May, 1872. 4to.
Mechanics' Magazine for May, 1872. 8vo.
Nature for May, 1872.
Pharmaceutical Journal for May, 1872. 8vo.
Photographic News for May, 1872. 4to.

Franklin Institute—Journal, No. 556. 8vo. 1872.

Linnean Society—Journal, No. 67. 8vo. 1871.
Proceedings, 1871-2. 8vo.

London Institution—Journal, No. 15. 8vo. 1872.

Meteorological Society—Quarterly Journal, No. 2. 8vo. 1872.

Noyes, Edward F. Esq. Governor of Ohio—Geological Survey of Ohio. Report for 1870. 8vo. 1871.

Photographic Society—Journal, No. 233. 8vo. 1872.

Preussische Akademie der Wissenschaften—Monatsberichte, Feb. 1872. 8vo.

Roberts, W. Chandler, Esq. M.R.I.—Second Annual Report of the Deputy-Master of the Mint. 8vo. 1872.

Royal Society of London—Proceedings, No. 134. 8vo. 1872.

Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, May, 1872. 8vo.

United Service Institution, Royal—Journal, No. 66. 8vo. 1872.

Zoological Society of London—Transactions, Vol. VII. Parts 7, 8. Vol. VIII. Part 1. 4to. 1872.
Proceedings for 1871, Parts 2, 3. 8vo. 1872.

WEEKLY EVENING MEETING,

Friday, June 7, 1872.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Professor Odling, F.R.S.

On the History of Ozone.

The most important points in the history of ozone are the following: I. Its recognition as a distinct variety of matter or substance, by Schönbein in 1840. II. An inquiry into its nature, made by Marignac in 1845, whereby it was established that the action of ozone on various substances results simply in their oxidation. III. The evidence of different kinds, accumulated by many observers during a period extending from 1845 to 1863, that the matter of ozone is identical with the matter of oxygen. IV. The demonstration by Andrews and Tait in 1860, that ozone is a condensed form of oxygen. V. The recognition by Andrews and Tait in 1860, and interpretation by the speaker in 1861, of the singular fact that, in certain cases, the removal of its constituent ozone from a mixture of ozone and oxygen is unattended by any alteration in the volume of the gas, notwithstanding the considerable oxidation effected by it. VI. The study of the quantitative reactions of ozone by Brodie in 1872; and his establishment of the relationship of ozone to ordinary oxygen, in corroboration of some less exact results obtained by Soret in 1865, as also of a suggestion made by the speaker in 1861.

I.

Ozone was discovered by Schönbein, in 1840, when experimenting with the then newly-invented battery of Sir Wm. Grove,—an instrument still recognized as yielding a current superior, in respect of joint quantity and intensity, to the current yielded by any other electromotor available for general use. Ozone was recognized by Schönbein successively, as a minute constituent of the oxygen gas resulting from the electrolysis of water effected by a current of high tension; as a minute constituent of air or oxygen through which electric discharges have taken place; and as a minute constituent of air in which moist phosphorus has been undergoing slow oxidation. To Schönbein then is due the great merit of recognizing ozone as a distinct form of matter, having an identity of its own by whatsoever means prepared—

as also the merit of discovering the most important means for the production of ozone, and of establishing its principal properties and reactions.

The general properties of ozone are those of an active oxygenant. Thus, like chlorine and peroxide of nitrogen, it bleaches colouring matters, corrodes fabrics, tarnishes or otherwise attacks metals, and liberates iodine from iodide of potassium. Its special properties are its characteristic pungent odour, its destructibility by a moderate heat, and its non-manifestation of any acidulous reaction.

II.

The nature of ozone was at first the subject of much speculation, Schönbein inclining to the view that it was a new elementary body, and a component of nitrogen. But in 1845, Marignac, in a series of most exact experiments, made partly in association with De la Rive, brought the question as to the nature of ozone within a very narrow compass. The experiments of these investigators, in which they established, among other points, that by exposure to the action of ozone, moist silver was converted simply into oxide of silver, and iodide of potassium into its oxidized form of iodate of potash, were susceptible only of one or other of two interpretations—either the interpretation which they themselves put on their results, that the matter of ozone is identical with the matter of oxygen—or, else the interpretation put on their results by Schönbein, that ozone is constituted of oxygen plus the elements of water, or in other words, that it is a peroxide of hydrogen. For a long time, experiment seemed quite incompetent to decide between these two views—opposite conclusions being arrived at almost alternately by the different investigators engaged on the inquiry. Corroboration, however, if any were needed, of the fact that ozone is really formed from oxygen itself with or without water, and not from any trace of nitrogen or other foreign matter that might possibly be present, was afforded by a remarkable experiment conducted by Fremy and Becquerel in 1853, being, indeed, the first recorded quantitative experiment made with ozone. By passing a long series of electric discharges through a given volume of oxygen standing over an aqueous solution of iodide of potassium, Messrs. Fremy and Becquerel succeeded in causing the whole of this oxygen to assume the form of ozone; as was shown by its ultimate complete absorption by the solution, with correlative liberation of iodine from the dissolved iodide of potassium.

The difficulty experienced in those early days of making out the real nature of ozone—of ascertaining whether it is a form of oxygen or a peroxide of hydrogen—depended mainly on the very small degree to which it was then possible to charge air or oxygen with the ozone to be examined, and on the necessity for the exclusive employment in the investigation of apparatus in which neither metal nor organic matter was present for the ozone to react with. The apparatus had

consequently to be constructed entirely of glass, and all the junctions to be made before the blow-pipe or by grinding. Now-a-days, by improvements in the methods of conducting the processes of electrization and electrolysis, it is possible to charge oxygen with ozone in very considerable proportion; while by means of paraffin, a substance on which ozone is without recognizable action, junctions of the glass apparatus employed may be made and unmade with the greatest facility.

III.

Assuming the ozone furnished by the three principal processes for its production to be one and the same substance, it was not until the year 1863 that the absolute freedom of ozone from any proportion of hydrogen was so definitely established as not to allow of any further question. In this year, Soret showed that although ozonized oxygen obtained by electrolysis, after having been desiccated as thoroughly as possible, frequently yielded some water as a product of its decomposition by heat, yet that when certain precautions were taken, and certain sources of error in the production and collection of the electrolytic oxygen were recognized and avoided, a uniformly negative result was obtained, and not a trace of moisture or other compound of hydrogen resulted from the decomposition by heat of the ozone present in the oxygen.

This conclusion of Soret's was confirmatory both of the previous result of Andrews with regard also to electrolytically obtained ozone, and of the yet earlier result of Schönbein himself with regard to the ozone obtained by the slow oxidation of moist phosphorus. For in opposition to the view enunciated first by himself, and in seeming discrepancy with the undoubted fact that for the production of ozone by means of phosphorus the presence of moisture is essential, Schönbein, in 1849, showed by repeated experiment, that when ordinary air in quantities of several hundred litres, ozonized as strongly as possible by its passage over moist phosphorus, was transmitted first through a desiccating tube, then through a tube heated to 400°, so as to effect the destruction of the ozone present, and finally through another desiccating tube to absorb any moisture that might result from the destruction of the ozone, this last desiccating tube did not show, by an increase of weight or other change, any absorption of moisture whatever, notwithstanding the largeness of the absolute quantity of ozone destroyed in the experiment. From this time forth, Schönbein abandoned the notion of hydrogen being a constituent of ozone; and while making a valid distinction between his own view and that of Marignac and De la Rive, admitted with them that the matter of ozone is identical with the matter of oxygen. These last-named investigators, in their research already referred to (1845), showed that perfectly dry oxygen, submitted to the influence of electric discharges, experienced an alteration of character, whereby it acquired the property of liberating iodine from moist iodide of potassium,—a result

afterwards confirmed by Fremy and Becquerel. But they did not regard this alteration of character as due to the formation in small proportion of a new substance within the mass of oxygen, but rather to the assumption by the mass of oxygen of a peculiar electric condition. Moreover, the fact of dry oxygen being capable of some modification by the action of electric discharges, coupled with the fact of the inability of the so modified oxygen to act upon iodide of potassium save in the presence of water, was not inconsistent with the notion of this modified oxygen having to unite with water in order to produce a compound identical with the ozone obtained immediately from moist or watery reagents. That the effect of electrical discharges, and more particularly of the silent discharge, on perfectly dry oxygen, is really to convert a small proportion of this oxygen into ozone identical with that furnished by electrolysis, and capable of acting upon certain substances, as mercury and iodine, when in the dry state, and on certain other substances, as iodide of potassium and metallic silver, only when in the moist state, was first put beyond question by Andrews and Tait, in a research next to be considered.

IV.

In the spring of 1860, Dr. Andrews and Prof. Tait made a joint communication to the Royal Society on the volumetric relations of ozone. The primary object of this research was to ascertain whether any, and if so what, alteration of volume took place in the conversion of a given quantity of oxygen into ozone. They thus attacked the problem from an entirely new point of view, and, with admirably directed pains and skill, succeeded in making probably the most important contribution hitherto made to an exact knowledge of the nature of the ozone. In their experiments, a quantity of perfectly pure and dry oxygen, contained in a straight glass tube with a pressure-gauge appendix, was ozonized by means of the silent electric discharge passed through the gas for some time. Coincidently with the passage of the silent discharge through it, the quantity of gas contained in the glass tube was observed to undergo a marked contraction in volume. This contraction proceeded at first rapidly, but afterwards more slowly, till it attained a limit which, in one of their experiments, was estimated at one-twelfth the original volume of the gas. And as whenever the gas, contracted in this manner, was examined, it was found to be proportionately ozonic, the general fact was established that the production of ozone from ordinary oxygen is attended with a contraction in volume. The converse result was also obtained. It was found that when oxygen, contracted by the passage of the electric discharge, was exposed for a short time to the temperature of 270°–300°, it was restored to its original volume. And as whenever the gas, re-expanded in this manner, was examined, it was found to be free from ozone, the general fact was established that the conversion of ozone into ordinary oxygen is attended with an expansion in volume. And

this alternate contraction of a given quantity of oxygen by exposure to prolonged electrization, with production of ozone, and re-expansion of the gas to its original volume by exposure to a temporary heat, with destruction of ozone, could be repeated an indefinite number of times. Now the only possible conclusion to be drawn from these experiments would appear to be that, the matter of ozone being identical with the matter of oxygen, ozone is oxygen in a denser form,—that is to say, in the form of a more complex unit. Some years afterwards, this conclusion was confirmed in a very interesting manner by Professor Tyndall, in the case of ozone obtained electrolytically. He found that the absorptivity for radiant heat of electrolytically obtained oxygen, when rich in ozone, was upwards of a hundred times greater than that of ordinary oxygen—a result indicating ozone to have a more complex molecular constitution, and consequently a greater density, than ordinary oxygen. Moreover, after this same electrolytically obtained and richly ozonic oxygen had been subjected to the action of heat, so as to have its ozonic character destroyed, it then exhibited merely the absorptivity for heat of ordinary oxygen,—the observed absorptivity not going at all beyond that of ordinary oxygen, as would have been the case if the ozone originally present in the electrolytic gas had been decomposed into ordinary oxygen and aqueous vapour.

Referring to the statement already made, that in Messrs. Andrews and Tait's experiments, the oxygen gas, more or less contracted by the electric discharge, was found to be proportionately ozonic, this point was ascertained in the following way:—A small thin glass bulb, containing a solution of iodide of potassium, was introduced into the oxygen-holding tube, prior to its being filled with the gas; which, after having been more or less contracted by the process of electrization, was next submitted to the action of the solution, released on the breaking, effected by concussion, of the small bulb wherein it was contained. And on estimating the quantity of iodine set free from the iodide of potassium solution by its reaction with the contracted gas, it was found to be the exact chemical equivalent of a weight of oxygen equal in volume to the amount of contraction which the original gas had experienced during the process of electrization; so that if in the process of electrization, there had been one, two, or three cubic centimètres of contraction, the quantity of iodine liberated was chemically equivalent to the weight of one, two, or three cubic centimètres of oxygen; whence it results that to ascertain the iodine-titre of the ozonized gas is to learn the contraction of the original gas effected by its electrization, or the correlative expansion of the electrized gas effected by its exposure to heat. In the case also of electrolytically obtained ozonized oxygen, it was shown firstly by Andrews and Tait, and subsequently by Soret, that the iodine-titre of the gas is the measure of its expansion by heat, consequent on the conversion of its constituent ozone into ordinary oxygen.

V.

It has just been remarked that in the action of the contracted gas on iodide of potassium solution, there is absorbed by the reagent, with equivalent liberation of iodine, a weight of oxygen corresponding to a volume equal to that of the original contraction; but very curiously, the absorption by the reagent of this weight of oxygen from the contracted gas was found by Messrs. Andrews and Tait not to produce any further contraction or alteration of its volume; or the weight of oxygen which acted on the iodide of potassium solution appeared to occupy no part of the volume of the contracted gas, its removal from the contracted gas by means of the reagent not effecting any alteration in that volume. Since this remarkable result was first announced by Messrs. Andrews and Tait in 1860, it has been abundantly confirmed by von Babo and Claus, by Soret, and by Sir Benjamin Brodie—the modes of experimenting adopted in the original investigation of Andrews and Tait and in the three subsequent investigations, being all different from one another. And moreover, not only has the fact been established by the four several investigations with regard to iodide of potassium, but by one or other of the investigations with regard also to iodine, to mercurous salts, to ferrous salts, to arsenites, and to ferrocyanides. So that, when a given volume of ozonized oxygen is allowed to act upon these different oxidizable bodies, the oxidation effected by the ozone present in the gas is found to be unattended by any diminution in the volume of the gas. An interpretation of this singular result was put forward by the speaker soon after the publication of Messrs. Andrews and Tait's experiments, to the following effect: Ozone being proved to be a condensed form of oxygen, it is clear that any volume of ozone will contain a greater weight of the matter of oxygen than is contained in the same volume of ordinary oxygen. And since in the action of ozone upon iodide of potassium, the volume of the reacting gas does not undergo any alteration, it is obvious that the oxidation effected must be effected by only so much of the matter of oxygen contained in the volume of ozone, as is in excess of the matter of oxygen contained in the same volume of ordinary oxygen. This interpretation, so far as it went, was considered to be demanded by Messrs. Andrews and Tait's experiments, as the only satisfactory explanation of them. With regard to the weight of the matter of oxygen contained in a given volume of ozone, in excess of the weight of the matter of oxygen contained in the same volume of ordinary oxygen, no data whatever existed to show what this weight really is. But relying upon the fact that the weight of oxygen contained in a standard volume of free oxygen is composed of two simple weights of the matter of oxygen, it was conjectured that the weight of oxygen contained in a standard volume of ozone might not improbably be constituted by an introduction into the standard volume of ordinary oxygen of another simple weight of oxygen—equal volumes of ozone, free oxygen, and nitric oxide, for example, being expressi[1]

by the comparable formulæ O_3, O_2 and NO respectively. In accordance with this supposition, the action of ozonized gas upon iodide of potassium, &c., is explicable as follows:—The ozone which acts is decomposed into a weight of free oxygen equal in volume to the volume of the ozone, and into another weight of absorbed oxygen, assumed to be one-half of the former weight. The suggestion as to the standard volume of ozone being constituted thus of three simple weights of the matter of oxygen, was admittedly not a necessary deduction from the then known facts, which were indeed equally consistent with its being constituted of four, five, or six such weights; it was only the suggestion of the simplest possible constitution for ozone that was consistent with the facts.

A few years afterwards, in 1865–66, the probability of this being the real constitution of ozone was much strengthened by the results of some experiments conducted by Soret. Operating by a process very simple and ingenious, but scarcely calculated to afford precise results, Soret found that when electrolytically obtained ozonized oxygen was allowed to act upon oil of turpentine, the absorption of the ozone by the turpentine was attended by a diminution in the volume of the gas equal approximatively to twice the initial contraction,—as inferred, of course, from the iodine-titre of the ozonized gas, or from its permanent expansion after exposure to a temporary heat. Supposing the final diminution effected by the turpentine to have been exactly twice the initial contraction, inferred from the iodine-titre, it is clear, that while the original gas would have suffered altogether a diminution of three volumes, the ozonized gas would have suffered a diminution of only two volumes. Or there would have been ultimately abstracted from the original uncontracted gas three volumes of the matter of oxygen, occupying in the contracted or ozonized gas, submitted to the action of the turpentine, only the bulk of two volumes. But as a mean result of Soret's first set of five experiments, the final diminution effected by the turpentine was 2·40 times the original contraction; while, as a mean result of his second set of seven experiments, the final diminution was 1·81 times the original contraction. Assuming, however, ozone to have the constitution expressed by the symbol O_3, its specific gravity, and consequently its diffusion-velocity, would approximate closely to the specific gravity and diffusion-velocity of carbonic acid gas, CO_2; and in 1867, Soret, in corroboration of his previous absorption results, satisfied himself that the diffusion-velocity of ozone really does approximate very closely to that of carbonic acid.

VI.

During the last few years, the quantitative reactions of ozone have been made the subject of an elaborate study by Sir Benjamin Brodie, whose results constitute indeed "a body of exact information as to the chemical properties of ozone, through which it may be hoped that this important question will be finally removed from the domain of

arbitrary speculation and brought within the precincts of science." In Brodie's experiments, a quantity of pure perfectly dry oxygen, after having been submitted to electrization by its passage through a modified form of Siemens' induction tube, carefully maintained at a low temperature, was collected in an oil of vitriol gas-holder, to the amount of four or five thousand cubic centimètres. From the store of ozonized gas thus collected, which was found to maintain its proportion of ozone without appreciable deterioration for some hours, portion after portion was allowed to pass into a pipette of about 250 c.c. capacity, by displacement of the oil of vitriol originally filling it; and successive equal volumes of the store of ozonized gas so measured off, were submitted one after another to the action of the same or of different reagents, by the passage of each pipetteful of gas through a small bulb-tube containing the reagent in solution. The gas, freed from all ozone by its passage through the reagent, was next received into a mercurial measuring cylinder, in which it was expanded to a definite volume; and then, by reading off the pressure at which it occupied this volume, its proper volume was ascertained. Finally, the difference between the capacity of the pipette and the volume obtained in the mercurial cylinder showed the volume of the gas absorbed by the reagent,—the weight of this gas being determined directly or indirectly by titrations of the reagent. Of course, very many points of detail in the construction and use of the apparatus had to be attended to, in order to ensure trustworthy results; but, both in its principle and general working, the process is exceedingly simple. One essential novelty consists in the application of that very useful instrument, the pipette, to the purposes of gas analysis; and by its aid, results having a degree of accuracy far greater than those furnished by any previous method of ozone investigation, were found to be obtainable with both speed and facility.

Operating, then, with the apparatus just described, Brodie has succeeded in establishing three perfectly definite classes of ozone-reactions,—the first of them including the instances previously made known by Andrews and Tait. In this first class of reactions, of which there are several distinct varieties, the absorption or decomposition of the ozone present in a mixture of ozone and oxygen, is unattended by any diminution in the volume of the gas; or a volume of ozone O^{2+x}, is resolved into an equal volume of free oxygen, and an indefinite weight of other oxygen, either absorbed or set free. This class of reactions accordingly does not afford any information as to the value of x, or, in other words, as to the relationship of the formula O^{2+x} to the formula O^2.

Unit-volumes.

$$\overbrace{O^{2+x}} = \overbrace{O^2} + x\,O \text{ absorbed.}$$

In a second class of reactions, the absorption or decomposition of the ozone present in a mixture of ozone and oxygen, is attende

a diminution in the volume of the gas, equal to half the volume that the weight of oxygen absorbed would occupy in the free state. In this class of reactions, then, one of but two occurrences must happen: either the ozone present in the mixture of gases, is absorbed wholly without decomposition, in which case the density of ozone must be twice that of ordinary oxygen, and the formula O^{2+x} become O^4; or the ozone present in the mixture, is decomposed into half its volume of oxygen liberated, and into a quantity of oxygen, corresponding to its entire volume, absorbed; in which case the density of ozone must be one-and-a-half that of ordinary oxygen, and the formula O^{2+x} become O^3.

Unit-volumes.

$$\left.\begin{array}{rcl} O^{2+x} & = & \text{nil} + 4\,O \\ 2\,(O^{2+x}) & = & O^2 + 4\,O \end{array}\right\} \text{absorbed.}$$

In a third class of reactions, the absorption or decomposition of the ozone present in a mixture of ozone and oxygen, is attended with a diminution in volume of the gas, equal to two-thirds the volume that the weight of oxygen absorbed would occupy in the free state; or the weight of the gas absorbed is to the weight of an equal volume of oxygen as 3 to 2. But consistently with this class of reactions, the density of ozone must necessarily be one-and-a-half times that of ordinary oxygen, and the formula O^{2+x} must become O^3.

Unit-volumes.

$$(O^{2+x}) = \text{nil} + 3\,O.$$

This last and most important class of reactions, by which the formula of ozone as O^3, is put beyond question, was established by a long series of experiments, made chiefly with a neutral or but slightly alkaline solution of hyposulphite of soda, and in a few cases with oil of turpentine. As a result, the ratio of the entire diminution in volume suffered by the original oxygen, to the diminution in volume of the electrized or contracted oxygen effected by the reagent, was found to be, as a mean of twenty-seven concordant experiments made with the hyposulphite, 3·02 to 2·02; and as a mean of eight concordant experiments made with the turpentine, also as 3·02 to 2·02. But neither with the hyposulphite nor with the turpentine, could the weight of oxygen absorbed by the reagent be determined, otherwise than by a calculation from the alteration in volume of the gas. A direct determination, however, was effected in the case of a few experiments made with protochloride of tin, under conditions carefully considered and regulated so as to ensure a trustworthy result. And it was found, in these few experiments, that the weight of the matter of oxygen absorbed from the ozonized gas by the tin-salt, was almost exactly three times the weight of the matter of oxygen absorbed from the same gas by iodide of potassium,—the volume occupied by the weight of oxygen absorbed by the tin-salt

being almost exactly twice the volume proper to the weight of oxygen absorbed by the iodide of potassium.

Independently of the importance attaching to the actual determination of the density of ozone, Sir B. Brodie's result has a further interest for chemists, which it would be difficult to exaggerate. The principal of the elementary bodies, known to chemists in the gaseous or vaporous state, are hydrogen, chlorine and its congeners, oxygen, sulphur, nitrogen, phosphorus, arsenic, mercury, and cadmium. Now it is a fact that the weight of phosphorus or arsenic contained in any volume of phosphorus or arsenic vapour, is four times the weight of phosphorus or arsenic contained in the same volume of phosphoretted or arsenetted hydrogen, and of a host of other phosphoretted or arsenetted gases or vapours. It is also a fact that the weight of hydrogen, chlorine, oxygen, or nitrogen contained in any volume of each of these elementary gases, is twice the weight of hydrogen, chlorine, oxygen, or nitrogen contained in the same volume of a variety of hydrogenous, chlorinous, oxygenous, or nitrogenous compound gases. It is also a fact that the weight of mercury or cadmium contained in any volume of the vapour of either element, is identical with the weight of the element contained in the same volume of the vapour of all its hitherto examined volatile compounds. But now a variety of oxygen is shown to exist, the weight of any given volume of which is three times the weight of oxygen contained in the same volume of the simplest of oxygenous compounds respectively, thus:—

Unit-volumes.			
P_4	O_3	O_2	Hg
PCl_3	CO	NO	$HgCl_2$.

The question, then, naturally arises, how long will it be before another variety of oxygen is recognized, the weight of any given volume of which, like that of a given volume of phosphorus vapour, shall furnish the weight of the element contained in four such volumes of its several simplest compounds? And again, how long will it be before yet another variety of oxygen is recognized, the weight of any given volume of which, like that of any given volume of mercury vapour, shall furnish but the weight of the element contained in the same volume of its several simplest compounds? There is the strongest indirect reason for believing in the existence of such a unitary oxygen. For in its reactions, oxygen behaves as a sort of more active electro-negative counterpart of electro-positive mercury Hg; and like mercury Hg, and unlike hydrogen H_2, and chlorine Cl_2, it enjoys the property of adding itself to a pre-formed unit of substance by an indivisible proportion.

[W. O.]

GENERAL MONTHLY MEETING,

Monday, July 1, 1872.

WILLIAM POLE, Esq. M.A. F.R.S. Manager, in the Chair.

Sir Henry Bouverie Paulet St. John Mildmay, Bart.

was *elected* a Member of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

Her Majesty's Government—Memoirs of the Geological Survey. Vol. IV. 8vo. 1872.
British Organic Remains. Decade 13. 8vo. 1872.
Actuaries, Institute of—Life Tables. 8vo. 1872.
Batavia Magnetical and Meteorological Observatory—Observations. Vol. I. folio. 1871.
Cape of Good Hope, Royal Observatory—Results of Observations in 1856. 8vo. 1871.
Chemical Society—Journal for June, 1872. 8vo.
Comitato Geologico d'Italia—Bollettini. 1872. Nos. 3, 4. 8vo. Firenze.
Editors—Academy for June, 1872. 4to.
American Journal of Science for May, June, 1872. 8vo.
Athenæum for June, 1872. 4to.
Chemical News for June, 1872. 4to.
Engineer for June, 1872.
Food Journal for June, 1872. 4to.
Horological Journal for June, 1872. 8vo.
Journal of Gas Lighting for June, 1872. 4to.
Mechanics' Magazine for June, 1872. 8vo.
Nature for June, 1872. 4to.
Nautical Magazine for July, 1872.
Pharmaceutical Journal for June, 1872. 8vo.
Photographic News for June, 1872. 4to.
Franklin Institute—Journal, No. 557. 8vo. 1872.
Geographical Society, Royal—Proceedings, Vol. XVI. No. 2. 8vo. 1872.
Lincoln's Inn, Hon. Society of—Third Supplement of Catalogue of the Library. 8vo. 1872.
Liverpool Literary and Philosophical Society—Proceedings, No. 25. 8vo. 1871.
Meteorological Office—Quarterly Weather Report. 1870. Part 4. 4to. 1872.
Payne, Joseph, Esq. (the Editor)—E. A. Youmans, Essay on the Culture of the Observing Powers of Children. 16mo. 1872.
Photographic Society—Journal, No. 234. 8vo. 1872.
Royal Society of London—Correspondence respecting the Great Melbourne Telescope. 8vo. 1871.
Astronomical and Magnetical and Meteorological Observations at Greenwich in 1870. 4to. 1872.
Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine, June, 1372. 8vo.
British Rainfall, 1871. 8vo. 1872.

Tyndall, John, Esq. LL.D. F.R.S. (the Author)—Fragments of Science. Fourth Edition. 8vo. 1872.
Contributions to Molecular Physics in the Domain of Radiant Heat. 8vo. 1872.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, Jan. und Feb. 1872. 4to.
Würzburg Physikal-Medecin. Gesellschaft—Verhandlungen, Band II. Heft 4. 8vo. 1871.
Yorkshire Archæological and Topographical Association—Journal, Part 5. 8vo. 1872.
Yorkshire Philosophical Society—Communications in 1872. 8vo. 1872.

GENERAL MONTHLY MEETING,

Monday, Nov. 4, 1872.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Henry Dominic Phillips, Esq.

was *elected* a Member of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM
Alcock, Col. M.R.I. (the Author)—The Relative Power of Nations. (K 98) 8vo. 1872.
Asiatic Society of Bengal—Journal, Part 1, 1871, No. 3; 1872, Part 2, No. 1. Proceedings, 1872, Nos. 2–5. 8vo.
Asiatic Society, Royal—Journal: New Series. Vol. VI. Part 1. 8vo. 1872.
Astronomical Society, Royal—Proceedings, Vol. XXXII. Nos. 8, 9. 1872.
Bavarian Academy of Sciences, Royal—Abhandlungen, Band XI. Abth. 1. 4to. 1871.
Sitzungsberichte. 1871; Heft 3. 1872; Heft 1.
Bombay Geographical Society—Transactions, Vol. XIX. Part 2. 8vo. 1871.
British Architects, Royal Institute of—Sessional Papers, 1871–2, Nos. 11, 12. 4to.
British Museum Trustees—Catalogue of Hemiptera Heteroptera, Part 5. 8vo. 1872.
Appendix to Catalogue of Shield Reptiles, Part 1. 4to. 1872.
Chemical Society—Journal for July–Oct. 1872. 8vo.
Church, Prof. A. H. M.A. M.R.I. (the Author)—Manuale di Chimica Pratica. 8vo. 1871.
Civil Engineers, Institution of—Minutes of Proceedings, Vol. XXXIII. XXXIV. 8vo. 1872.
Clinical Society—Transactions, Vol. V. 8vo. 1872.
Comitato Geologico d'Italia—Bollettini. 1872 Nos. 5, 6. 8vo. Firenze.
Commissioners in Lunacy—26th Report. 8vo. 1871.
Croll, James, Esq. (the Author)—What determines Molecular Motion? (From Phil. Mag., July, 1872.) 8vo.
Dawson, J. W. Esq. LL.D. F.R.S. (the Author)—Notes on the Post-pliocene Geology of Canada. 8vo. 1872.

Editors—American Journal of Science for July–Oct. 1872. 8vo.
Athenæum for July–Oct. 1872. 4to.
Chemical News for July–Oct. 1872. 4to.
Engineer for July–Oct. 1872.
Food Journal for July–Oct. 1872. 4to.
Horological Journal for July–Oct. 1872. 8vo.
Journal of Gas Lighting for July–Oct. 1872. 4to.
Mechanic's Magazine for July–Oct. 1872. 8vo.
Nature for July–Oct. 1872. 4to.
Nautical Magazine for July–Oct. 1872. 8vo.
Once a Week for July–Oct. 1872. 8vo.
Pharmaceutical Journal for July–Oct. 1872. 8vo.
Journal of Applied Science for July–Oct. 1872. Folio.

Elliot, The Lady—The History of India as told by its own Historians. By Sir H. M. Elliot. Vol. IV. 8vo. 1872.

Enderby, Charles, Esq. F.R.S. M.R.I. (the Author)—Treatise on Capital, Money, and Riches. 8vo. 1872.

Franklin Institute—Journal, Nos. 558–561. 8vo. 1872.

Geographical Society, Royal—Proceedings, Vol. XVI. No. 3. 8vo. 1872.

Geological Institute, Imperial, Vienna—Jahrbuch, 1872. Nos. 1, 2. 8vo.
Verhandlungen, 1872. Nos. 1–10. 8vo.
Abhandlungen, Band V. Heft 3. 4to. 1872.

Geological Society—Quarterly Journal, No. 111. 8vo. 1872.

Glasgow Philosophical Society—Proceedings, Vol. VIII. No. 1. 8vo. 1872.

Harvey, Augustus J. Esq.—Tabular View of the Orders and Families of Insects.

Incorporated Law Society of the United Kingdom—Catalogue of the Mendham Collection. 8vo. 1871.

Iron and Steel Institute—Journal, Nos. 1–3. 8vo. 1872.

Linnean Society—Journal, No. 55. 8vo. 1872.
Proceedings, 1872. 8vo.
Transactions, Vol. XXVIII. Part 2; Vol. XXIX. Part 1. 4to. 1872.

Manchester Philosophical Society—Proceedings, Vol. XI. No. 14, Vol. XII. No. 1. 8vo. 1872.

Mechanical Engineers' Institution, Birmingham—Proceedings, May, 1872. 8vo.

Medical and Chirurgical Society, Royal—Proceedings, Part 35. 8vo. 1872.

Meteorological Committee of the Royal Society—Report for 1871. 8vo. 1871.

Meteorological Office—Quarterly Weather Report, 1871. Part 1. 4to. 1872.
Meteorology of the Atlantic, with Charts. 4to. 1872.

Meteorological Society—Quarterly Journal, No. 3. 8vo. 1872.

Philadelphia Academy of Natural Sciences—Proceedings for 1871. 8vo. 1871–2.

Pirala, Don Antonio (the Author)—El Rey en Madrid y en Provincias. 8vo. Madrid. 1872.

Plateau, Professor J. Hon. M.R.I. (the Author)—La Mesure des Sensations Physiques. (Bull. de l'Acad. de Belgique, Tome XXXIII.) 8vo.

Preussische Akademie der Wissenschaften—Monatsberichte, April, May, 1872. 8vo.

Roma, Reale Accademia dei Lincei—Atti, Tomo XXV. Sess. 1–3. Fol. 1872.

Ross, Major W. A. (the Author)—Pyrology, or Fire Analysis. (R. S. Proc., No. 137, 1872.) 8vo.

Royal Society of London—Proceedings, Nos. 135, 136, 137. 8vo. 1872.
Philosophical Transactions for 1871, Part 2. 4to. 1872.

Smithsonian Institution, U.S.—Annual Report, 1870. 8vo. 1872.

Société Hollandaise des Sciences—Archives Néerlandaises, Tome VII. Nos. 1–3. 8vo. 1871–2.

Statistical Society—Journal, Vol. XXXV. Parts 2, 3. 8vo. 1872.

St. Pétersbourg, Académie Impériale des Sciences—Mémoires, 7e Série, Tome XVII. Nos. 11, 12; Tome XVIII. Nos. 1–7. 4to. 1871–2.
Bulletins, Tome XVII. Nos. 1, 2. 4to. 1871–72.

Symons, G. J. Esq. (the Author)—Symons' Monthly Meteorological Magazine. July–Oct. 1872. 8vo.
Telegraph Engineers, Society of—Journal, Vol. I. No. 1. 8vo. 1872.
Tyndall, Professor, LL.D. F.R.S. (the Author)—In den Alpen. 8vo. Braunschweig. 1872.
The Forms of Water in Clouds and Rivers, Ice and Glaciers. 16to. 1872.
United Service Institution, Royal—Journal, No. 67. 8vo. 1872.
Upsal Royal Society of Sciences—Nova Acta, Series III. Vol. VIII. Fasc. 1. 4to. 1871.
Bulletin Météorologique, Vol. I. 1-12; Vol. II. 1-12; Vol. III. 1-12. 4to. 1870-1.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, Marz-Juni. 1872. 4to.
Vernon, Wm. F. Esq. M.R.I. (the Author)—Notes on Harefield, Middlesex. 4to. 1872.
Victoria Institute—Journal of Transactions, Vol. VI. No. 23. 8vo. 1872.
Wells, T. Spencer, Esq. F.R.C.S. (the Author)—Diseases of the Ovaries. 8vo. 1872.
Wild, M. H.—Repertorium für Meteorologie, Band II. Heft 2. 4to. 1872.
Würzburg Physikal-Medecin. Gesellschaft—Verhandlungen, Band III. Heft 1, 2. 8vo. 1871.
Yorkshire Archæological and Topographical Association—Journal, Parts 6, 7. 8vo. 1872.
Zoological Society of London—Transactions, Vol. VIII. Part 2. 4to. 1872.
Proceedings, 1872, Part 1. 8vo.
List of Vertebrated Animals in the Gardens. 8vo. 1872.
Catalogue of the Library. 8vo. 1872.

GENERAL MONTHLY MEETING,

Monday, Dec. 2, 1872.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Thomas Thompson, jun. Esq.
Sidney Waters, Esq.

were *elected* Members of the Royal Institution.

The following LECTURE ARRANGEMENTS for the ensuing season were announced:—

Christmas Lectures (adapted to a Juvenile Auditory).

PROFESSOR ODLING, M.A. F.R.S. Six Lectures On Air and Gas, on Dec. 28 (Saturday), Dec. 31, 1872; Jan. 2, 4, 7, 9, 1873.

Before Easter, 1873.

PROFESSOR RUTHERFORD, M.D. F.R.S.E. Twelve Lectures On the Forces and Motions of the Body, on Tuesdays, Jan. 14 to April 1.

DR. DEBUS, F.R.S. Three Lectures On Oxidation, on Thursdays, Jan. 16, 23, 30.

DR. H. E. ARMSTRONG, F.C.S. Four Lectures On the Artificial Formation of Organic Substances, on Thursdays, Feb. 6 to Feb. 27.

A. VERNON HARCOURT, Esq. M.A. F.R.S. Five Lectures On the Chemistry of Coal and its Products, on Thursdays, March 6 to April 3.

EDWARD A. FREEMAN, Esq. D.C.L. Six Lectures On the Comparative Political Institutions of Different Nations, on Saturdays, Jan. 18 to Feb. 22.

PROFESSOR W. K. CLIFFORD, M.A. Three Lectures On the Philosophy of the Pure Sciences, on Saturdays, March 1, 8, 15.

PROFESSOR MAX MÜLLER, LL.D. Three Lectures On Darwin's Philosophy of Language, on Saturdays, March 22, 29, and April 5.

After Easter.

JOHN MORLEY, Esq. Three Lectures On the Limits of the Historic Method, on Tuesdays, April 22 to May 6.

J. H. PARKER, Esq. C.B. Four Lectures On the Evidence for the Traditional History of Rome from Existing Architectural Remains, on Tuesdays, May 13, 20, 27, and June 3.

PROFESSOR TYNDALL, LL.D. F.R.S. Six Lectures, on Thursdays, April 24 to June 5.

PROFESSOR ODLING, M.A. F.R.S. Four Lectures, on Saturdays, April 26 to May 17.

EDWARD DANNREUTHER, Esq. Three Lectures On the Development of Music in Connection with the Drama, on Saturdays, May 24, 31, and June 7.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Lords of the Admiralty—The Nautical Almanac for 1876. 8vo. 1872.
Agricultural Society, Royal—Journal, 2nd Series, Vol. VIII. Part 2. 8vo. 1872.
Asiatic Society of Bengal—Journal, 1872, Part 2, No. 2. 8vo.
Proceedings, 1872, Nos. 6, 7, 8. 8vo.
Atchison, Surgeon-Major T. (the Author)—Timely Counsel. 8vo. 1872.
British Architects, Royal Institute of—Sessional Papers, 1872-3, No. 1. 4to.
Chemical Society—Journal for Nov. 1872. 8vo.
Editors—American Journal of Science for Nov. 1872. 8vo.
Athenæum for Nov. 1872. 4to.
Chemical News for Nov. 1872. 4to.
Engineer for Nov. 1872.
Food Journal for Nov. 1872. 4to.
Horological Journal for Nov. 1872. 8vo.
Journal of Applied Science for Nov. 1872. Folio.
Journal of Gas Lighting for Nov. 1872. 4to.
Mechanic's Magazine for Nov. 1872. 8vo.
Nature for Nov. 1872. 4to.
Nautical Magazine for Nov. 1872. 8vo.
Once a Week for Nov. 1872. 8vo.
Pharmaceutical Journal for Nov. 1872. 8vo.
Telegraphic Journal, No. 1. 8vo. 1872.
Falconer, Thomas, Esq. (the Editor)—W. Falconer: Dissertation on St. Paul's Voyage from Cæsarea to Puteoli. 8vo. New Ed. 1872.
Franklin Institute—Journal, No. 562. 8vo. 1872.
Geological Society—Quarterly Journal, No. 112. 8vo. 1872.
Manchester Philosophical Society—Proceedings, Vol. XII. No. 2. 8vo. 1872.
Nomentino, F.R.G.S.—Nuova Italia: a Poem. 16to. 1872.
Nystrom, J. W. Esq. (the Author)—On the Force of Falling Bodies. (L 15) 8vo. 1872.
Odontological Society of Great Britain—Transactions. New Series. Vol. IV. 8vo. 1872.
Photographic Society—Journal, No. 235. 8vo. 1872.
Preussische Akademie der Wissenschaften—Monatsberichte, Juni, Juli, 1872. 8vo.

Rennie, James, Esq. F.R.S. M.R.I.—J. M. Chelius. System of Surgery: Translated by J. F. South. 2 vols. 8vo. 1845–7.

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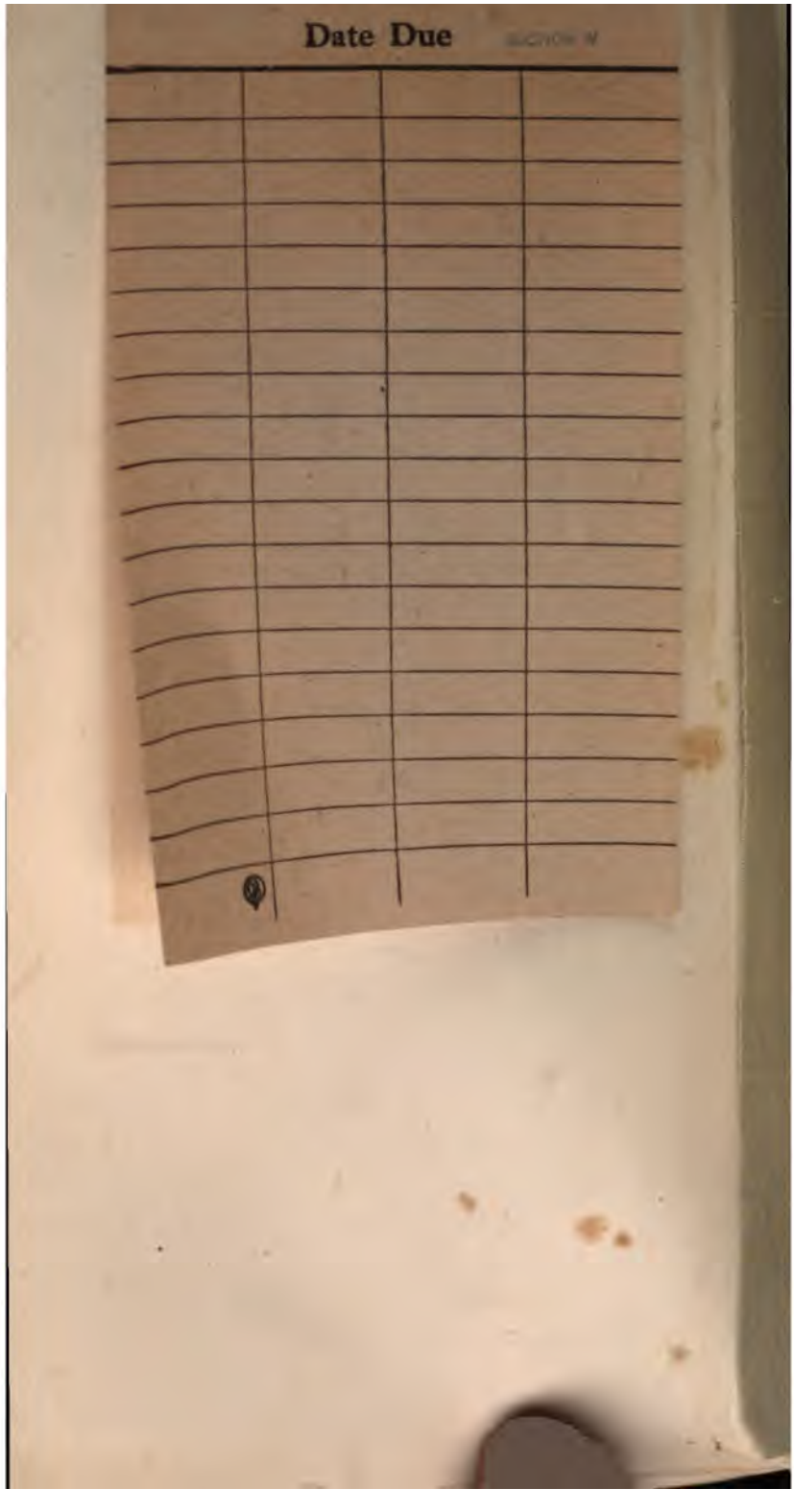
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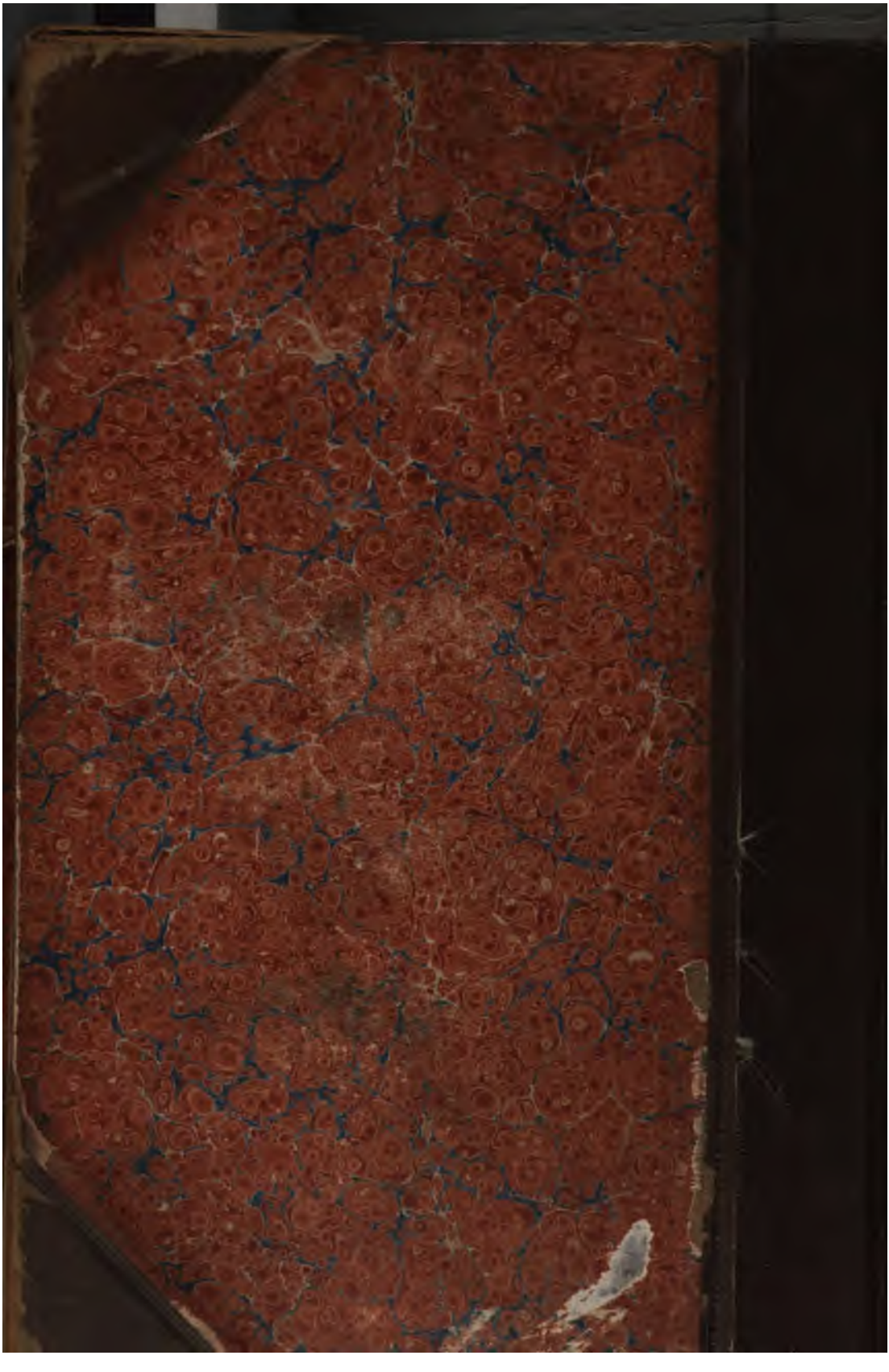
Just wanted to tell you I WAS HERE! Hope you are having a fabulous School year. Don't worry about anything b/c you are one of the smartest and friendliest people I know. I miss you a lot and if you ever need ANYTHING or just feel like talking I'm just a phone call away.

Marisa

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